

TRANSACTIONS

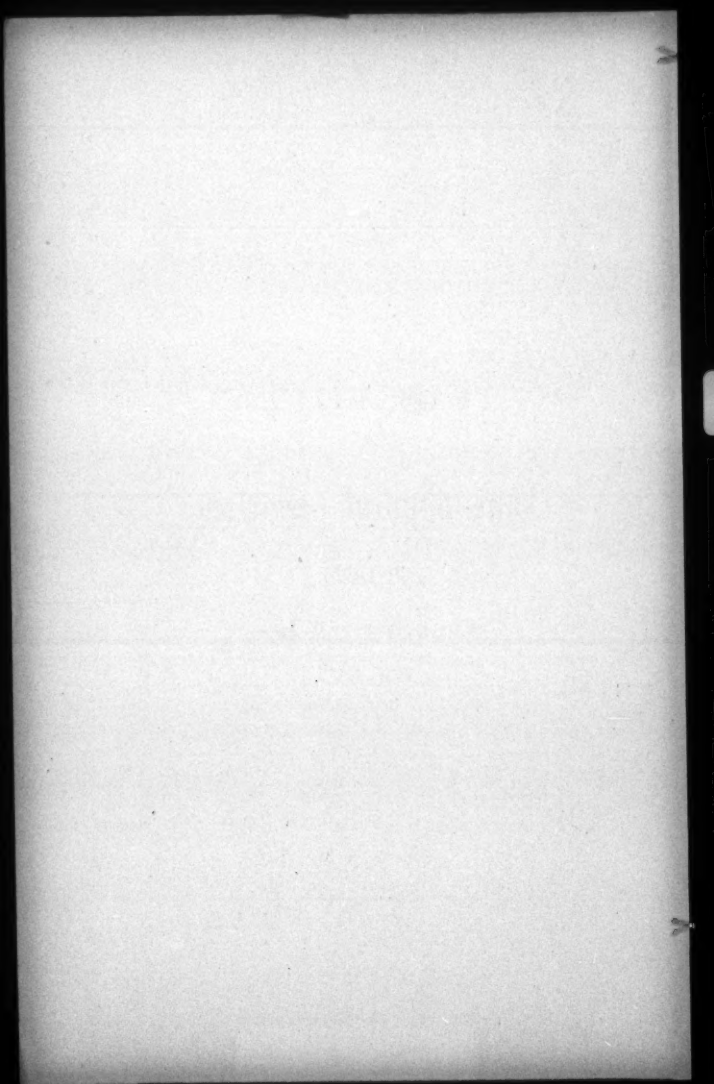
OF

The Optical Society

LONDON.

SESSION 1914-1915.

VOL. XV.



CONTENTS.

	PAGE
I.—THE POSSIBLE EFFECTS OF THE WAR ON BRITISH OPTICS. T. H. BLAKESLEY	5
II.—OPHTHALMOLOGICAL INSTRUMENT DESIGN WITH SPECIAL REFERENCE TO ACCURACY AND RAPIDITY OF WORKING. PRESIDENTIAL ADDRESS BY DR. W. ETTLES	35
III.—THE DESIGN, DETAILS OF CONSTRUCTION, AND USE OF TRENCH PERISCOPES. WALTER A. DIXEY ...	78
IV.—TESTING FOR ASTIGMATISM. DR. R. S. CLAY ...	99
V.—INSTRUMENTS IN GENERAL USE FOR THE MEASUREMENT OF SOLAR RADIATION. R. S. WHIPPLE ...	106
VI.—FLUOR-GLASS CONCENTRIC EYE-PIECE FOR HIGH POWERS. LT. COL. J. WILLIAM GIFFORD ...	188
VII.—A REVISED SYSTEM OF COMPARATIVE OPTICS. LT. COL. J. WILLIAM GIFFORD	187
VIII.—SOME POINTS IN CONNECTION WITH THE DESIGN OF SEARCHLIGHTS AND PROJECTORS, AND THE VISIBILITY OF DISTANT ILLUMINATED OBJECTS. J. S. DOW	216
IX.—NOTE ON THE ACHROMATISM OF A PAIR OF SEPARATED LENSES. T. B. VINYCOMB	244
X.—OPTICAL ACCESSORIES. WILLIAM SALT	256

1884

1. The first of the year was a very cold one, with much snow and ice. The weather was very disagreeable, and the people were much distressed. The crops were all killed, and the people were forced to live on their stocks. The winter was very severe, and the people suffered much. The spring was also very cold, and the crops were all killed. The summer was very hot, and the people suffered much. The autumn was very cold, and the people suffered much. The year was a very bad one for the people, and they were all very poor.

2. The second of the year was a very cold one, with much snow and ice. The weather was very disagreeable, and the people were much distressed. The crops were all killed, and the people were forced to live on their stocks. The winter was very severe, and the people suffered much. The spring was also very cold, and the crops were all killed. The summer was very hot, and the people suffered much. The autumn was very cold, and the people suffered much. The year was a very bad one for the people, and they were all very poor.

3. The third of the year was a very cold one, with much snow and ice. The weather was very disagreeable, and the people were much distressed. The crops were all killed, and the people were forced to live on their stocks. The winter was very severe, and the people suffered much. The spring was also very cold, and the crops were all killed. The summer was very hot, and the people suffered much. The autumn was very cold, and the people suffered much. The year was a very bad one for the people, and they were all very poor.

4. The fourth of the year was a very cold one, with much snow and ice. The weather was very disagreeable, and the people were much distressed. The crops were all killed, and the people were forced to live on their stocks. The winter was very severe, and the people suffered much. The spring was also very cold, and the crops were all killed. The summer was very hot, and the people suffered much. The autumn was very cold, and the people suffered much. The year was a very bad one for the people, and they were all very poor.

5. The fifth of the year was a very cold one, with much snow and ice. The weather was very disagreeable, and the people were much distressed. The crops were all killed, and the people were forced to live on their stocks. The winter was very severe, and the people suffered much. The spring was also very cold, and the crops were all killed. The summer was very hot, and the people suffered much. The autumn was very cold, and the people suffered much. The year was a very bad one for the people, and they were all very poor.

INDEX TO SUBJECTS.

IN THIS INDEX THE VOLUMES FROM 1899-1900 TO 1914-1915
ARE NUMBERED I. TO XV.

(VOLUME XIII. COMPRISES 1911-1912 AND 1912-1913.)

(VOLUME XIV. COMPRISES THREE SEPARATE PARTS.)

Aberration Effects, Spherical	S. D. Chalmers ..	XIII. 29
Aberrations	Prof. S. P. Thompson ..	I. 16
Aberrations of Photographic Lenses, Measurement of	S. D. Chalmers ..	VII. 86
Abrasives and Polishing Materials ..	Walter Rosenhain ..	XI. 112
Absorption in Tinted Glasses, Measure- ment of	L. W. Phillips ..	VI. 43
Accessories, Optical	W. Salt ..	XV. 256
Accommodation of the Eye, Theories of	H. L. Taylor ..	VII. 106
Accommodation and Convergence in		
Ametropia, Relationship between	L. Laurance ..	II. 47
Achromatism of a Pair of Separated Lenses	T. B. Vinycomb ..	XV. 244
Advantages and Disadvantages of Objec- tive and Subjective Methods of Sight Testing	W. J. Wright ..	X. 5
Advertising for Opticians, Collective or Co operative	H. F. Purser ..	XIV. 46
Ametropia, Relationship between Accom- modation and Convergence in ..	L. Laurance ..	II. 47
Artificial Lighting, The Evolution of ..	C. V. Drysdale ..	VIII. 33
Astigmatism in Lens System	S. D. Chalmers ..	XII. 146
Astigmatism, Testing for	R. S. Clay ..	XV. 99
Astigmometer	H. M. Selfe ..	II. 59
Astronomical Eye-pieces, Berthonian System of Standardising	J. J. Hall ..	IV. 41
Astronomical Instruments, On the Con- struction of	Sir David Gill ..	XIII. 64
Berthonian System of Standardising Astronomical Eye-pieces	J. J. Hall ..	IV. 41
Bifocal Lenses, Fused	V. H. Mackinney ..	X. 41
Bimocular Vision and the Theory of the Stereoscope	T. W. Barber ..	V. 145
Bilsh Prism	H. A. Hughes ..	XIV. 60
Calculations, Practical Optical	T. Smith ..	XIII. 196
Centering of Spectacle Lenses	W. A. Dixey ..	I. 48
Collective or Co-operative Advertising for Opticians	H. F. Purser ..	XIV. 46
Colour Perception, Theory of Vision and Colour, Production of, by Colourless Media	F. W. Edridge Green ..	XI. 10
Colour Photography, Some Modern De- velopments in	R. C. Cann-Lippincott ..	VII. 96
Colour Vision	A. J. Bull ..	IV. 90
Colour, the Measurement of and the Sensitiveness of the Eye to small Changes in Hue	G. T. Burch ..	IX. 107
	F. Stanley ..	XIII. 184

Colour Photography for Opticians ..	H. A. Boatman and H. C. Patey ..	XIII. 30
Comparative Optics, A Revised System of	Lt.-Col. J. W. Gifford ..	XV. 187
Compass and Sextant, A New	H. A. Hughes ..	XIII. 38
Condensers, The Use of Mirrors as ..	S. D. Chalmers ..	XIII. 68
Convergence and Accommodation in Ametropia, Relationship between ..	L. Laurance	II. 47
Concomitant Esotropia, The Optical Treatment of	W. Ettles	V. 1
Co-operation, Education, Standardisation Curvature of Small Lenses, Method of Determining	R. T. Glazebrook	V. 184
Curvature of Small Lenses, On the Measurement of the	C. V. Drysdale	II. 1
Cylindrical Lenses, High Power	S. D. Chalmers ..	XIV. 83
Cylindrical Lenses, Method of Testing Spherical and	H. L. Taylor	IX. 73
	C. V. Drysdale	I. 72
Definition, Resolving Power and, in Optical Instruments	R. T. Glazebrook ..	IX. 94
Design of Optical Instruments, Some Points in	C. V. Drysdale	IV. 11
Development of the Science of Physio- logical Optics in the 19th Century ..	Dr. Tscherning	IX. 1
Discourse on Films	J. T. Sandell	III. 39
Direct Stereoscopic Projection	T. Brown	VI. 10
Education of the Optician	W. A. Dixey	II. 35
Education, Standardisation, Co-operation Effect of Illumination on Acuteness of Vision	R. T. Glazebrook	V. 184
Effects of Distance on High Power Sphero Cylindrical Lenses	J. S. Dow	XI. 124
Efficiencies of Photographic Shutters ..	S. D. Chalmers ..	VIII. 170
Entoptic Vision	N. de Graaf Hunter ..	VIII. 1
Esotropia, The Optical Treatment of Concomitant	Prof. Barrett	VII. 184
Evolution of Artificial Lighting	W. Ettles	V. 1
Experiments with Selenium	C. V. Drysdale	VIII. 38
Eye-pieces, Astronomical, Berthsonian System of Standardising	A. Rosenberg	IV. 1
Eyeglasses and Spectacles, on the Evolution of	J. J. Hall	IV. 41
	M. W. Dunscombe ..	XIII. 115
Face Measurement, Spectacle Frame Fitting and	W. A. Dixey	II. 13
Fatigue Observations	C. V. Burton	XIV. 41
Field Glasses, Telescopes and	Dr. Kaempfer	II. 21
Films, Discourse on	J. T. Sandell	III. 39
Fitting, Spectacle Frame, and Face Measurement	W. A. Dixey	II. 13
Fluor-Glass Concentric Eye-piece for High Powers	Lt.-Col. J. W. Gifford ..	XV. 183
Focus of a Lens, On the	J. W. Gordon	XI. 31
Focus of a Lens, A Discussion on Certain Theoretical Questions in Relation to the	F. J. Selby	X. 81

iii.

Focusing and Lens Measurement, Principles of	T. H. Blakesley ..	I. 64
Frame Fitting, Spectacle and Face Measurement	W. A. Dixey ..	II. 13
Functions of Tricolour Filters	A. J. Bull and A. C. Jolley ..	V. 31
Fused Bifocals	V. H. Mackinney ..	X. 41
German Progress, The Secret of	H. F. Angus ..	III. 31
Gifford's Method of Measuring Refractive Indices, Note on	S. D. Chalmers ..	XIV. 57
Glass, Some Properties of	W. Rosenhain ..	IV. 98
Glass, Thermometer	Dr. Harker ..	VIII. 51
Glass, Some Notes on Optical	S. Lamb ..	XIV. 67
Glasses, Telescope and Field	Dr. Kaempfer ..	II. 21
High Power Sphero-Cylindrical Lenses	S. D. Chalmers ..	VIII. 170
High Power Cylindrical Lenses	H. L. Taylor ..	IX. 72
Horizon, Paget Artificial	H. A. Hughes ..	XIV. 59
Hunter's Lens Testing Apparatus, Demonstrations of	T. Smith ..	XI. 5
Hygrometry	W. A. Dixey ..	IV. 50
Illumination on Acuteness of Vision, The Effect of	J. S. Dow ..	XI. 124
Improvements in the Methods of Trial Case Testing at the National Physical Laboratory	F. J. Selby ..	X. 21
Instrument Design, Ophthalmological Instruments, Definition and Resolving Power in Optical	W. Ettles ..	XV. 35
Instruments, Some Points in the Design of Optical	R. T. Glazebrook ..	IX. 24
Instruments, Testing of Optical	C. V. Drysdale ..	IV. 11
Instruments, Some new Optical	C. V. Drysdale ..	III. 70
Instruments, Some New Optical	Max Poser ..	XIII. 21
Interim Reports	C. V. Drysdale ..	XIV. (Pt. 2) 19
Iris Diaphragm, The Theory of the	C. F. Lan Davis ..	IV. 8
Lens Measurement, Principles of Focusing and	C. F. Lan Davis ..	XII. 113
Lens, On the Focus of a	T. H. Blakesley ..	I. 64
Lens System, Astigmatism in	J. W. Gordon ..	XI. 31
Lens Testing Apparatus, Demonstrations of Mr. Hunter's	S. D. Chalmers ..	XII. 146
Lenses, Centering of Spectacle	T. Smith ..	XI. 5
Lenses, High Power Sphero-Cylindrical	W. A. Dixey ..	I. 48
Lenses, High Power Cylindrical	S. D. Chalmers ..	VIII. 170
Lenses, Measurement of Aberrations of Photographic	H. L. Taylor ..	IX. 72
Lenses, Method of Determining Curvature of Small	S. D. Chalmers ..	VII. 86
Lenses, On the Measurement of the Curvature of Small	C. V. Drysdale ..	II. 1
Lenses, On Some New Forms of Spectacle	S. D. Chalmers ..	XIV. 82
Lenses, Periscope	Dr. Klinghardt ..	XIV. 101
Lenses, Spherical and Cylindrical, Method of Testing	W. A. Dixey ..	VI. 1
	C. V. Drysdale ..	I. 72

iv.

Lenses, The Testing of Photographic ..	S. D. Chalmers ..	V. 87
Light Rays, Wireless Telegraphy by Means of ..	A. Rosenberg ..	IV. 1
Light Projection, The Optics of ..	V. H. Mackinney ..	IX. 124
Light by Living Beings, On the Production of ..	Max Coque ..	XIII. 34
Light, On the Polarisation of ..	S. P. Thompson ..	XIII. 1
Lighthouses ..	W. Rosenhain ..	VI. 29
Literature, Early, of Optics ..	S. P. Thompson ..	VI. 63
Magnalium ..	Dr. Kaempfer ..	II. 32
Measurements of Stereoscopic Power, On some ..	D. P. Boatman and R. J. Lucking ..	XI. 107
Measurement, Spectacle Fitting and Face	W. A. Dixey ..	II. 13
Measurement of Aberrations of Photographic Lenses ..	S. D. Chalmers ..	VII. 86
Measurement of Absorption in Tinted Glasses ..	L. W. Phillips ..	VI. 43
Method of Determining Curvature of Small Lenses ..	C. V. Drysdale ..	II. 1
Mica, Optical and Physical Properties of	S. P. Thompson ..	III. 1
Mica, On the Mining, Preparation and Uses of ..	H. P. Wiggins ..	XIII. 93
Micrometer, a New Form of Vernier	J. W. Gordon ..	XIII. 1
Mirrors as Condensers, The Use of ..	S. D. Chalmers ..	XIH. 63
Naval Purposes, Optical Instruments for ..	T. Y. Baker ..	XI. 65
Nature of Vision, Some Notes on the ..	A. J. Bull ..	VI. 76
Near Work, Necessity for Reforms in Spectacles for ..	Dr. Berger ..	III. 21
Necessity for Reforms in Spectacles for Near Work ..	Dr. Berger ..	III. 21
Notation, Prisms and their ..	L. Laurance ..	III. 120
National Physical Laboratory, The Optical Society and ..	R. T. Glazebrook ..	V. 56
National Physical Laboratory, On Some Improvements in the Methods of Trial Case Testing at the ..	F. J. Selby ..	X. 21
Northampton Institute, The Optical Classes at the ..	Dr. Walmsley ..	VII. 61
Objective and Subjective Methods of Sight Testing, Advantages and Disadvantages of ..	W. J. Wright ..	X. 5
One Position Ophthalmometry ..	J. H. Sutcliffe ..	VIII. 74
Ophthalmic Apparatus ..	W. Ettles ..	XII. 160
Ophthalmic Instruments ..	H. S. Ryland ..	XII. 105
Ophthalmological Instruments, Demonstration of ..	Reiner ..	XII. 4
Ophthalmological Instrument Design with Special Reference to Accuracy and Rapidity of Working ..	W. Ettles ..	XV. 35
Ophthalmometer, The ..	W. Ettles ..	VIII. 253
Ophthalmometry, One Position ..	J. H. Sutcliffe ..	VIII. 74
Optical Accessories ..	W. Salt ..	XV. 256
Optical Classes at the Northampton Institute ..	B. M. Walmsley ..	VII. 61

Optical Glass, Some Notes on	S. Lamb	XIV. 67
Optical Instruments, Some New	C. V. Drysdale	XIV. (Pt. 2) 19
Optical Instruments for Naval Purposes	T. Y. Baker	XI. 65
Optical Instruments, Testing of	C. V. Drysdale	III. 70
Optical Instruments, Some Points in the Design of	C. V. Drysdale	IV. 11
Optical Instruments, Notes on Resolving Power and Definition in	R. T. Glazebrook	IX. 94
Optical and Physical Properties of Mica	S. P. Thompson	III. 1
Optical Projection Apparatus	F. J. Cheshire	IX. 154
Optical Society and the National Phys- ical Laboratory	R. T. Glazebrook	V. 56
Optical Standards	L. Laurance	I. 4
Optical Standards	W. Salt	VII. 17
Optical Treatment of Concomitant Esotropia	W. Ettles	V. 1
Optics in the Application	F. Kendall	V. 108
Optics of Light Projection	V. H. Mackinney	IX. 194
Optician, The Education of the	W. A. Dixey	II. 35
Paget Artificial Horizon	H. A. Hughes	XIV. 59
Periscopic Lenses	W. A. Dixey	VI. 1
Periscopes, Design, Details of Construc- tion and Use of Trench	W. A. Dixey	XV. 78
Photographic Lenses, Testing of	S. D. Chalmers	V. 87
Photographic Lenses, Measurement of Aberrations of	S. D. Chalmers	VII. 86
Photographic Shutters, A New Method of Measuring the Times and Efficiencies of	N. de Graaf Hunter	VIII. 1
Photographic Shutters at the National Physical Laboratory, Results of Some Tests on	T. Smith	XII. 128
Physical and Optical Properties of Mica	S. P. Thompson	III. 1
Physiological Optics, Development of the Science of, in the 19th Century	Dr. Tscherning	IX. 1
Polarisation of Light, on the	S. P. Thompson	XIII. 1
Polished Surfaces, Researches on	G. H. Beilby	IX. 22
Polishing Materials, On Abrasives and Practical Optical Calculations	Walter Rosenhain	XI. 112
Precious Stones, The Properties of	T. Smith	XIII. 196
Prescriptions for Spectacles, On the Difficulties met with in filling	B. J. Tully	XI. 96
Prism, The Blish	W. Green	XIII. 124
Prisms and their Notation	H. A. Hughes	XIV. 60
Prisms, A Method of Testing	L. Laurance	III. 130
	S. D. Chalmers and H. S. Ryland	VI. 34
Principles of Focussing and Lens Meas- urement	T. H. Blakesley	I. 64
Projection, The Optics of Light	V. H. Mackinney	IX. 194
Projection, Direct Stereoscopic	T. Brown	VI. 19
Projection Work, Recent Advances in Optical	R. S. Wright	XIII. 162
Production of Colour by Colourless Media	R. C. Cann-Lippincott	VII. 96
Presidential Address	W. H. H. Thornthwaite	I. 1
	R. M. Walsley	I. 32
	W. A. Dixey	II. 35
	C. Hyatt-Woelf	III. 49

Presidential Address	C. V. Drysdale ..	IV. 63
	R. T. Glazebrook ..	V. 134
	S. P. Thompson ..	VI. 63
	R. M. Walmsley ..	VII. 61
	W. Rosenhain ..	VIII. 103
	S. D. Chalmers ..	XII. 141
	Sir David Gill ..	XIII. 64
	W. Etiles ..	XV. 35
Properties of Precious Stones	B. J. Tully ..	XI. 26
Radiation, Instruments in General Use for the Measurement of Solar ..	R. S. Whipple ..	XV. 106
Reflector for Illuminating Flat Surfaces, A New Form of	V. H. Mackinney and S. D. Chalmers ..	XIII. 1
Refractive Indices, Note on Mr. Gifford's Method of Measuring	S. D. Chalmers ..	XIV. 57
Relative Value of Parts in Spectacle Frame Manufacture	H. L. Taylor ..	IX. 76
Relationship Between Accommodation and Convergence in Cases of Ametropia	L. Laurance ..	II. 47
Reports, Interim		IV. 8
Report of Standards Committee on Stan- dardisation of Trial Cases		IX. 138
Researches on Polished Surfaces ..	G. H. Beilby ..	IX. 22
Resolving Power and Definition in Optical Instruments, Notes on ..	R. T. Glazebrook ..	IX. 94
Searchlights and Projectors, The De- sign of	J. S. Dow ..	XV. 216
Sextant, A New Compass and	H. A. Hughes ..	XIII. 28
Sextant, On the Design of a	T. Y. Baker ..	XIV. 86
Sight Testing Advantages and Disad- vantages of Objective and Subjec- tive Methods	W. J. Wright ..	X. 5
Selenium, Experiments with	A. Rosenberg ..	IV. 1
Small Lenses, Method of Determining Curvature of	C. V. Drysdale ..	II. 1
Solar Radiation, Instruments for the Measurement of	R. S. Whipple ..	XV. 106
Spectacle Frame Fitting and Face Measurement	W. A. Dixey ..	I. 48
Spectacle Frame Manufacture, Relative Value of Parts	H. L. Taylor ..	IX. 76
Spectacle Lenses, Centering of	W. A. Dixey ..	I. 48
Spectacle Lenses, On Some New Forms of	Dr. Klughardt ..	XIV. 101
Spectacles, Stereoscopic	Dr. Berger ..	III. 31
Spectacles, On the Evolution of Eye- glasses and	M. W. Duncombe ..	XIII. 115
Spectacles, Difficulties met with in Filling Prescriptions for	W. Green ..	XIII. 124
Spectrometer, A New, Its Uses and Advantages	V. H. Mackinney ..	VII. 84
Spherical and Cylindrical Lenses, Method of Testing	C. V. Drysdale ..	I. 72
Spherical Aberration Effects	S. D. Chalmers ..	XIII. 29

Spharo-cylindrical Lenses, Effect of Distance on High Power	S. D. Chalmers	VIII. 170
Standardisation, Co-operation, Education	R. T. Glazebrook	V. 134
Standardisation of Astronomical Eye-Pieces	J. J. Hall	IV. 41
Standards, Optical	L. Laurance	I. 4
Stereoscope, Binocular Vision and the Theory of	T. H. Barber	V. 145
Stereoscopic, Direct, Projection	T. Brown	VI. 10
Stereoscopic Power, On Some Measurements of	D. P. Boatman, and R. J. Lucking	XI. 107
Stereoscopic Spectacles	Dr. Berger	III. 21
Stones, The Properties of Precious	B. J. Tully	XI. 26
Stroboscopy	C. V. Drysdale	VII. 1
Surface Brightness and a New Instrument for its Measurement	J. S. Dow and V. H. Mackinney	XII. 66
Surface Protection of Metals, Some Processes for	H. S. Ryland	X. 16
System, Berthonian, of Standardising Astronomical Eye-pieces	J. J. Hall	IV. 41
Telephony, Wireless, by Means of Light Rays	A. Rosenberg	IV. 1
Telescope Constructions, Some Modern	A. R. Hinks	XIII. 7
Telescopes and Field Glasses	D. Kaempfer	II. 21
Testing of Optical Instruments	C. V. Drysdale	III. 70
Testing of Photographic Lenses	S. D. Chalmers	V. 87
Testing of Spherical and Cylindrical Lenses, Method of	C. V. Drysdale	I. 72
Testing of Trial Cases	F. J. Selby	VIII. 137
Testing Prisms, A Method of	S. D. Chalmers and H. S. Ryland	VI. 43
Theories of the Accommodation of the Eye	H. L. Taylor	VII. 106
Theory of Vision and Colour Perception	F. W. Edridge Green	XI. 10
Thermo Piles as applied to Optical Instruments, Recent Developments in	L. Bellingham	XIII. 179
Thermometer Glass	J. Harker	VIII. 51
Thomas Young Oration	Dr. Tscherning	IX. 1
Thomas Young Oration	Prof. R. W. Wood	XII. 22
Thomas Young Oration	Sir J. Crichton-Browne	XIV. Pt. 2
Tinted Glasses, Measurement of Absorption in	L. W. Phillips	VI. 43
Trench Periscopes	W. A. Dixey	XV. 78
Trial Cases, Testing of	F. J. Selby	VIII. 137
Trial Cases, Report of the Standards Committee on the Testing of		IX. 188
Trial Case Testing at the National Physical Laboratory, On Some Improvements in the Methods of	F. J. Selby	X. 21
Tricolour Filters, Function of	A. J. Bull and A. C. Jolley	V. 31
Visibility of Distant Illuminated Objects	J. S. Dow	XV. 216
Vision, Effect of Illumination on Acuteness of	J. S. Dow	XI. 124
Vision, Entoptic	Prof. Barrett	VII. 124

viii.

Vision, Some Notes on the Nature of ..	A. J. Bull	VI.	76
Vision, Theory of, and Colour Perception ..	F. W. Edridge Green ..	XI.	10
Visual Phenomena, Some new	F. W. Edridge Green ..	XIII.	10
Visual Optics	J. C. Eberhardt ..	XIV. (Pt. 1)	1
War, the possible Effects of the, on			
British Optics	T. H. Blakesley ..	XV	5
Wireless Telephony by Means of Light			
Rays	A. Rosenberg	IV.	1
Work of the Optical Society	R. T. Glazebrook ..	V.	134
Workshop Problems and Processes ..	F. Kendall	V.	108

INDEX TO NAMES OF AUTHORS AND SPEAKERS.

Abney, Sir W.	..	VIII. 22, 25.
Adams, S. B.	..	XIII. 149.
Aitchison, J.	..	II. 44; V. 129, 143; VI. 73; VIII. 154.
Aitchison, I.	..	XIV. 44.
Angus, H. F.	..	III. 31.
Baker, T. Y.	..	XI. 65, 103; XIII. 225; XIV. 42, 61, 64, 86, 136; XV. 29, 93.
Barber, T. W.	..	V. 145.
Barrett, W. F.	..	VII. 134.
Beck, C.	..	V. 103; VIII. 23; XI. 94.
Beck, H.	..	V. 105; VI. 42, 75; VIII. 28.
Beilby G. T.	..	IX. 22.
Bellingham, L.	..	XIII. 179.
Bentzon, M.	..	VIII. 157; XV. 20.
Berger, Dr.	..	III. 21.
Blakesley, T. H.	..	I. 64; V. 105; XI. 98; XV. 5, 29, 35.
Boatman, A. W.	..	VI. 7.
Boatman, D. P.	..	XI. 107.
Boatman, H. A.	..	XIII. 30.
Brown, T.	..	VI. 10.
Browne, Sir J. Crichton	..	XIV. Pt. 2.
Bryan, G. B.	..	XV. 234, 235.
Bull, A. J.	..	IV. 90; V. 31, 106; VI. 76.
Burch, G. J.	..	IX. 107.
Burton, C. V.	..	XIV. 41.
Butler, —	..	XIII. 9.
Cann-Lippincott, R. C.	..	VII. 96.
Chalmers, S. D.	..	V. 75, 87, 123; VI. 31, 34; VII. 86, 165; VIII. 29, 50, 71, 73, 156, 170; XI. 20, 101, 103, 137, 139, 139; XII. 16, 17, 19, 21, 146; XIII. 1. 19, 29, 63, 92, 175, 282; XIV. 42, 43, 44, 82, 124, 128, 130, 132, 133, 139; XV. 23, 68, 95.
Cheshire, F. J.	..	IX. 154.
Chree, Dr.	..	XV. 172.
Clarke, A. B.	..	VI. 7.
Clay, B. S.	..	V. 49; XI. 56; XII. 12, 14, 15, 20; XV. 74, 97, 99.
Connolly, T. F.	..	XIV. 43, 34, 45; XV. 209, 236, 238, 254.
Coque, Max	..	XIII. 3.
Curties, C. L.	..	VIII. 70, 102.
Dadd, F. W.	..	XV. 102.
Dixey, W. A.	..	I. 43; II. 13, 35; III. 65; IV. 35, 61, 88, 121; V. 26, 30, 127, 164, 166; VI. 1, 42, 55, 59; VII. 23, 28, 30, 33, 131; VIII. 102, 150, 158, 160, 163, 167, 172; XI. 135, 137; XII. 7, 12, 15, 16, 17, 19; XIV. 43, 45, 126, 130, 131, 133, 138, 140; XV. 21, 69, 75, 78, 96, 103.

I.

Dow, J. S.	XI. 21, 124, 138; XII. 86; XV. 216, 238, 241, 242.
Drysdale, C. V. ..	I. 72; II. 1; III. 70; IV. 11, 60, 63, 96, 122; V. 28, 48, 52, 73, 81, 101, 108; VI. 42, 74; VII. 1; VIII. 33; XIV. 128, Pt. 2; XIV. 19.
Duncombe, M. W. ..	VI. 6; XIII. 115.
Dyson, Sir F.	XV. 170.
Eberhardt, J. C. ..	XIV. 1, Pt. 1.
Eck, J.	XV. 292.
Edridge-Green, F. W. ..	XI. 10, 23, 193; XIII. 10.
Edser, Dr.	VII. 164.
Etiles, W.	V. 1, 52, 79; VI. 88; VII. 123, 132, 161; VIII. 53; XI. 134; XII. 160; XIV. 124, 132, 137, 139, 140; XV. 15, 31, 75, 97, 103, 169, 202, 206, 215, 231, 238.
Everett, J. D.	IV. 118.
Fairbanks, H. R. ..	XIV. 65.
Firth, H.	XIV. 43, 65.
Gamble, ...	V. 50.
Garnett, Dr.	VI. 56.
Garnett, J. C. M. ..	VI. 58, 62.
Gaster, L.	VIII. 46.
Glazebrook, R. T. ..	V. 56, 101, 127, 130, 134, 166; VI. 58, 72, IX. 94.
Gifford, Lt.-Col J. W. ..	XV. 183, 187, 203, 206, 209, 210, 212, 213, 215.
Gill, Sir David	XIII. 64.
Gold, E.	XV. 178.
Gordon, J. W.	VII. 163; XI. 31, 59; XIII. 1; XV. 204.
Green, W.	XIII. 124; XIV. 133, 140.
Grey, A. E.	IV. 97; VII. 25, 32, 33; VIII. 102.
Haddon, C.	IV. 120.
Hall, J. J.	IV. 41.
Harker, J. A.	VIII. 51.
Henderson, Prof. ..	XI. 92.
Heywood, A.	V. 28.
Hinks, A. B.	XIII. 7.
Hopwood, H. V. ..	V. 165; VI. 31.
Hughes —	XII. 15; XIII. 18.
Hughes, H. A.	XIII. 28, 152; XIV. 59, 63, 64, 66.
Hunter, J. de G. ..	VIII. 1, 157.
Hyatt-Woolf, C. ..	III. 49; IV. 49, 87; V. 142, 143; VI. 9, 98; VIII. 102 (as Chairman VII. 28, 33).
Jolley, A. C.	V. 31; VI. 91.
Kaempfer, D.	II. 21, 32.
Kendall, F.	IV. 121; V. 76, 108; VI. 87.
Klughardt, Dr. ..	XIV. 101.
Lamb, S.	XIV. 67; XV. 15, 212.
Lan-Davis, C. F. ..	XII. 113.
Last, E.	XV. 214.
Laurance, L.	I. 4; II. 47; III. 102.

Lowry, Dr.	XV. 207.
Lucking, R. J.	XI. 108.
Mackinnay, V. H.	IV. 122; V. 80; VII. 34; VIII. 50, 154, 173; IX. 124; X. 41; XI. 130; XII. 12, 14, 15, 66; XIII. 1; XIV. 126, 183.
Marks, D.	VI. 9.
Marriott, W.	XV. 175.
Patey, H. C.	XIII. 30.
Phillips, L. W.	VI. 48.
Poser, Max	XIII. 23.
Prentice, C.	VIII. 102.
Purser, H. F.	V. 107; VI. 7; VII. 25, 32, 33; VIII. 25 XIII. 180; XIV. 46, 129, 131, 138; XV. 94, 95.
Reiner, J. B.	XI. 97; XII. 4, 18, 19, 20.
Ritchie, T. E.	XV. 235, 242.
Rosenberg, A.	IV. 1; VIII. 48.
Rosenbain, W.	IV. 98; VI. 92; VIII. 26, 71, 73, 102, 105, 158; XI. 112; XIII. 93, 174; XV. 210.
Ryland, H. S.	V. 52, 81, 130; VI. 34; VIII. 30, 158, 159; X. 16; XI. 138; XII. 105.
Salt, W.	III. 45; IV. 37; VII. 17, 30, 32; XV. 256.
Sandell, J. T.	III. 39.
Selby, F. J.	V. 105; VII. 27; VIII. 137; X. 21; XI. 55.
Selke, H. M.	II. 59.
Shackleton, W.	XIV. 63, 64; XV. 94.
Smith, T.	XI. 5, 58, 100, 188; XII. 123; XIII. 192, 196, 228; XIV. 65; XV. 202, 214.
Stanley, F.	XIII. 184.
Sutcliffe, J. H.	IV. 61; VI. 7; VIII. 74, 152; XIII. 147.
Swithinbank, C. W.	XI. 97.
Taylor, H. L.	VII. 106; IX. 72, 76.
Thompson, S. P.	I. 16; III. 1; VI. 63; XIII. 2.
Thornthwaite, W. H. H.	I. 1; IV. 48, 60, 121.
Tscherning, M. H. E.	IX. 1.
Tully, B. J.	XI. 26.
Vinycomb, T. B.	XV. 244, 254.
Walsmsley, B. M.	I. 32; II. 44; III. 67; IV. 37, 119; V. 51, 74, 80, 143; VI. 31, 32, 73, 93; VII. 21, 23, 56, 61; VIII. 30, 158 (as Chairman VII. 123, 161); XIII. 18; XV. 17, 203.
Whipple, F. J. W.	XV. 177.
Whipple, B. S.	XV. 106, 181.
Williams, Major	XV. 18, 89, 95.
Wiggins, H. F.	XIII. 98.
Wood, B. W.	XII. 22.
Wooley, ...	III. 43.
Wright, W. J.	X. 5.
Wright, B. S.	XIII. 162.

THE POSSIBLE EFFECTS OF THE WAR ON BRITISH OPTICS.

BY PROF. T. H. BLAKESLEY, M.A.

Paper read 12th November, 1914.

AN enormous historical event, such as is the present war, cannot fail to produce changes of some sort in the various currents of human life all the world over. Disturbances will be felt in Honolulu as in Hatton Garden. They will vary in magnitude, and may be for the general good of civilisation or the reverse. It is unlikely that they will be in one direction only. The fruits of Peace, Commerce, and Philanthropy, Science and Justice, Patriotism and Enthusiasm, may be stimulated or repressed, as the case may be. It is to be hoped that indifference and carelessness, prejudice and ignorance, which may be called the weeds of peace, may be diminished and tempered. Much may depend upon the fortune of war, but changes must be awaited in any event:

No one who has had his eyes open for the last thirty or forty years can fail to be aware of the enormous progress which has attended the efforts of the Germans in matters of commerce and manufacture.

When I was a boy, growing up in the quiet country house in which Henry Coddington had lived and worked at the improvement in optical science, it was a common thing to see in the cartoons of 'Punch,' John Bull upon his island surrounded by smoking factory chimneys, and contemplating, with an easy complacency, his mills and his monopolies.

The broad and romantic stream of the Rhine

was as peaceful and clear as the Thames at the pumping stations of the London Metropolitan Waterworks, and was navigated by small paddle steamers, on board of which you could hear directions given in English, of "Ease her," "Back her," "Stop her."

In 1851 and 1862 occurred the two international exhibitions, of which England was so proud. To-day the Rhine is crowded with long trains of enormous barges and other craft, and the Rhineland itself is thickly studded with factories, showing every sign of the greatest commercial activity and prosperity. And this success is apparent throughout the whole of Germany.

Our exhibition buildings, when not occupied by roundabouts, are filled with the wounded and the refugee, closer evidence of the technics of war.

The position of Germany's optical glass trade was not produced in a moment by the wand of an enchanter, at the whim of a politician. Pains and study, work and thought, have been necessary to obtain for Germany those large and valuable industries which we in England at one time possessed, and by these means alone can they be recovered to any extent. This war may give us the opportunity, but to benefit by it we must open our eyes to the causes of things.

To-night we are more particularly concerned with the progress of British optics than in dye-stuffs and drugs, the treatment of steel, and the preparation of high explosives. But influences of a general character, such as belief in the profession of perfection, prejudice in favour of a foreign practice, will have their bad effects upon all lines of industry.

I have elsewhere taken occasion to point out that in the manufacture of optical glass England at one time occupied a very high place. Probably in the production of optical surfaces this was also the case.

When Newton first promulgated his theory of colours, on the strength of his experiments with prisms, his views were not at once received in France as true. In that country M. Mariote essayed to repeat the experiments and failed to reproduce Newton's results. Only after Cardinal de Polignac had procured prisms from England was Newton's experience satisfactorily repeated in France.

English flint glass was at the bottom of Dollond's success in achromatising the objectives of telescopes. It seems probable that had Euler been in possession of this material the investigations which he is known to have made in the theory, would have conducted him to the practical discovery. As it was he was compelled to experiment with water between lenses. If he had been able to procure carbon bisulphide he would almost certainly have been successful. The highly refracting material such as could be obtained in England, was necessary, a matter clearly apprehended by M. Clairaut and M. Anthéaume in France, when the latter sought to apply the rules of theory supplied by the former.

In that branch of opticians' work which deals with accurate angular measurements, the English were very successful. Graham made the sector by means of which Bradley detected the aberration of light, and also that which the French employed in measuring terrestrial arcs. 1736-1740.

Later Ramsden constructed most exquisite quadrants and mural circles for many places in Europe, in addition to inventing the eyepiece which bears his name, remarkable for its excellence in more ways than one. It approaches to the fulfilment of the sine law very much more nearly than the Huyghen's eyepiece, though, of course, that way of expressing its merits was not yet formulated.

The two important epochs at which Germany has made strides in optical theory and prac-

tice which have rivetted the attention and secured the admiration of the rest of the world, are first the time of Fraunhofer, and secondly, that of Schott and Abbé, respectively in the first and last quarters of last century. During the greater part of that century the objectives made upon Fraunhofer's pattern were in vogue, and are, indeed, even now perhaps to be met with as often as any others. They represent skill in theory working with limited materials. They are now yielding before the new varieties of form which theory has suggested to Abbe, produced with the new varieties of glass which the prolonged experiments of Schott have made profitable.

The consideration of the two events is very instructive, if taken in connection with the works of Newton, Dollond, and Euler, in the past, and with our hopes and prospects for the future.

Early Optical Glasses.

Up to Newton's time the theory of the composition of light established by him was unsettled, and no great advance could be made except empirically. Newton's hasty generalisation of proportionality in refraction, could only have been due to the very small variety of glasses which could be made of sufficient uniformity for optical use, and was at his disposal. Dollond was in possession of the denser glasses, which seem first to have appeared in England. His contemporary, Euler, was in possession of sufficient theory, but did not possess, or perhaps know of, the existence of the heavier glasses. On the other hand M. Anthéaume, already furnished by M. Clairaut with achromatic theory, took the trouble to obtain or reproduce the English types of glass.

Fraunhofer was, indeed, alive to the fact that improvement might be obtained with a greater variety of glass, and it is recorded that he made experiments in that direction, so that he must be

credited with a sense of the importance of both sides of the question. He, however, died young, at the age of 39. Without means himself, he was fortunate in having an enthusiastic friend in Utzschneider, an established optician in Benedictbeuern, and afterwards in Munich, who not only defrayed the cost of Fraunhofer's education, but encouraged him by taking him into his workshops and in other ways. Long after Fraunhofer's death the firm of Utzschneider continued to reap the benefit of this happy and creditable connection of talent and practice. But for this co-operation Fraunhofer might never have had his opportunity, and the firm might have stagnated in obscurity.

Schott and Abbe.

In the case of the work at Jena, we see the same set of forces in operation, but on a much larger scale. On the one side are the skill, the theory, and the enthusiasm of Schott and Abbe, on the other the appreciation, the intelligence, and the wealth of the Prussian government. The road to be travelled was a long one at best. Trials of vast numbers of ingredients, with great differences in proportions, had to be undertaken, contests with colouring devitrification and crucible reaction entered upon, defects in transparency, uniformity, and stability encountered and overcome.

All this work must be done in a philosophic way: each experiment, whatever its actual result, giving light and guidance for subsequent trials, must be recorded exactly.

In the end (which is not the end) this perseverance has had its reward in the present prosperity of the Jena firms, the welfare of the Jena University, and the splendid return upon the intelligent outlay of money, to the Government which provided it. The real knowledge attained, enormous as it must be, is still increasing. The idea of attained perfection finds no place.

If we admit that we have lost ground to make up, if we indulge in the hope that as a sequel to this war England may build up an optical industry commensurate with the German, the only course is an expenditure of thought and work.

Germany has, indeed, freely published her catalogues of results, from which a designer of instruments can choose his glasses. But the purchaser is none the wiser on the subject of the processes of manufacture of those glasses. The pure optician may raise no objection to this, but it certainly ought not to enter into the intentions of any body of Englishmen bent upon a great and lasting extension of optical industry, to leave out of consideration the important subject of manufacture of the glass itself, with all the various niceties and artifices in mixing and melting to ensure success. Chemical proportions are necessary, and are comparatively easily ascertained from successful examples, but the modes of dealing with the ingredients in the proper way, may not be so easily discovered.

And even if all this knowledge, so far as it has already gone in Germany, were in the possession of Englishmen, there is still much to be done in directions in which the Germans have not as yet met with complete success. For example, the borate and phosphate glasses from which much is to be hoped, were on account of their imperfect durability, it is presumed, excluded at least for a time from the Jena lists.

To refrain from these investigations in England is to wait upon foreign nations for what they may choose to give us of their results.

Assuming the matter of glass manufacture to be set upon the proper step of the moving staircase of progress, we have next to meet the difficulties of the quantitative determinations of the forms of surfaces required for specific purposes, that is the design or chamber work, and finally we must study the mechanics, of the tools which

are to produce the surfaces required and specified, again a factory matter.

Ideal Optical Glass.

I have dwelt with emphasis upon the glass manufacture; and perhaps I may state in brief that the object in view is to obtain uniform glass possessing indices which can be varied *continuously inter se*, and absorbing properties of *continuous* variability, at one or more points. These are ideal conditions, to be approached step by step more and more nearly, though never reached, like the image in a collimator.

The department of design will move onwards with improvement in the glasses on the one hand and with the improvement of factory tools on the other, and in the latter connection an important step would be the invention and employment of mechanical means of producing a departure, in predetermined measure, of surfaces from the strictly spherical. That such surfaces can be truly produced, I believe. That when produced they would be of enormous value in dealing with the subject of spherical aberration, I have by calculation satisfied myself. And that such a power over spherical aberration would afford a latitude in dealing with further matters, must be evident to those who consider the degrees of freedom in lens designing.

It must have occurred to many besides myself that the spherical form has nothing intrinsically necessary for image formation beyond the facts that it is a surface of revolution, cuts its axis at right angles, and is continuous. Besides these points, which are not peculiar to it, it has been supposed to be more easily constructed in truth than other shapes. This is an opinion founded upon old practice, and the tools of the present day. As a matter of fact, the final empirical finishing off of large lenses is, I believe, so constantly necessary and advantageous, that it is deemed a part of the process of production. Such a course I interpret as a practical admis-

sion of the fact that a departure from the spherical is often advantageous. Then why not let the matter be dealt with in the design?

Conical Lens Surfaces.

For some optical purposes it would be an advantage to produce highly finished conical surfaces. At one time I was endeavouring to procure an instrument involving a surface of this sort, but I could not obtain any reasonable guarantee of truth in its execution. I have also a word to say about that department of optical work which appertains to the design, the calculation shop, if I may use the expression. In dealing with the calculations of aberrations in lens designs, the calculator has constantly to deal accurately with very small numerical matters, the differences perhaps of two numbers a million times as large as this difference. Now mathematical tables (which are the tools of the computer) to seven places, as I believe is the usual case, are hardly capable of sustaining through a long and complex calculation the accuracy desirable for optical work. The tercentenary of the production of Napier's tables of natural logarithms was celebrated this year in Scotland with a marked amount of national pride and fervour. The tercentenary of the first instalment of Briggs' logarithms to the base 10, takes place in 1916. The extraordinary author was the first and most industrious of the Gresham Professors of Geometry. His industry has been the foundation of all exact calculation from his own time to the present, and it has in its extent of decimal places anticipated the needs of optical work in the immediate future. But these tables are not properly available, as they exist only in manuscript at the British Museum. Is it too much to hope that the Joint Grand Gresham Committee will remedy this state of things, by printing and issuing their invaluable documents, or such parts of them as are required, in the tercentenary year of 1916?

Considering these three departments of the optical trade we may enquire whether in England we have or have not the proper means of securing the continual satisfactory and sufficiently rapid progress. If the machinery already existing for technical advancement were adequate, why, I ask, has it occurred to your President to suggest the subject of to-night's discussion?

It is true that there is hardly a trade or profession which does not possess its confederation of brotherhood, whether it be a trades union or a scientific society. There have been public or semi-public associations for the nominal advancement of knowledge. Such have been the Royal Society of London for real knowledge, the Royal Society of Arts for encouragement, and the City and Guilds of London Technical Institute for technical purposes, in any useful direction. We have various universities, engineering societies, technical committees, and a State department of education. Many of these have done work of a moderate character, but few have maintained the reputation of their foundation aims.

Technical and Academic Requirements.

Some of these are quite unsuited to carry on such researches as I have mentioned, others have defeated the intention at foundation by becoming the teaching grounds of already established elementary knowledge, and schools of a university. In such cases numbers bring fees and obtain diplomas, but progress is made to give way to curriculum, within the narrow walls of which examiners have to be careful to keep. I have known cases where false views are given out as truths in the books of instruction, and are propagated in examination questions, the governing body being quite incompetent to detect such things.

The fact is that technical exactitude has to be far higher than academic requirements as

they are understood in England, and the reduction to university rank is fatal to a technical institution. Neglect of its technical aims has not merely been a pull back to progress, but has been the cause of disaster and loss of life.

What is wanted in England is some method of encouraging the national spirit of invention and ingenuity in practical matters. That were surely a wiser and more honourable course than to send officers in the British Army and Navy to spy into foreign arsenals and dockyards.

I am sincerely glad on this occasion to recall the fact that the British Science Guild has shown itself alive to the hindrances to optical advancement existing in this country. It has appointed a committee to consider the means of remedy, and I heartily hope that the recommendations this committee may see fit to make will be crowned with success. That they will be so crowned will depend upon the steadfastness with which the causes of failure in the past are kept in view. The *Idola Tribus*, as Bacon would say, of the British, must be given up, the German infatuation, the idea of the possibility of perfection in anything technical, the reception of untested authority, the masquerading in Charlottenburg buskins.

Care must be taken to keep free from the lower, unco-ordinated interests, and from those who like jackdaws in a sepulchre would treat the subject as a game and not as a gospel.

Will this war serve to break our national prejudices sufficiently to allow us to see what course we must take for the advancement of our industries, and will the peace to follow give us the means of thought and work in which alone progress is to be found?

Or shall we fall back again into the lethargic condition of dreamy academic self-complacency? Politicians and journalists may affect to answer these questions with all the speciousness of their professions. I cannot do so. All that I

can venture to say is that the enormous upheaval of all planes of equilibrium produced by this Besserk war should afford an opportunity for new lines of action to supersede those which have not been found effective. These lines may not be more successful, but we have more light to go by.

Discussion.

The PRESIDENT said they had listened to a very calm, judicious, and carefully-reasoned address by Mr. Blakesley, who had raised various points, and had dealt gently—(laughter)—with several national shortcomings. Some of the questions raised were of enormous interest, as for example the possible supply, or shortage of supply, of optical glass. He would be glad if any members or others present could tell them what the possible resources of this country were in regard to the material upon which optical work could be done, and perhaps it would be useful to have some observations from teachers of optics as to the lines which they would like to see followed were they given a free hand. They would also welcome the contributions of manufacturers who were constantly meeting the practical difficulties that everyone must experience who essayed to put abstract ideas into concrete form in regard to the optical industry.

MR. LAMB said the question of the manufacture of optical glass was so great that it would be altogether out of place for him to attempt to go into its intricacies and details on that occasion. The success achieved by the Schott works was largely due to the encouragement given by the Prussian Government, which was of great assistance to him in his endeavours to produce these exceedingly expensive kinds of glass. It must be remembered that no experiment of that kind could take place in a glass works under £50 to £100. Another great contributing factor to the success of the German works (and he was now speaking from his own experience and

not on behalf of the firm he had the honour to represent) was the co-operation of that remarkable genius Abbe, because it was not only a question of making glass, but of applying it when made. Glasses had been made in this country in almost innumerable quantities and of almost innumerable types, but those glasses had not been applied, for reasons which he would not attempt to go into that evening. The question was one not only of making the glass but of applying it, and that was where Abbe's work was of the very greatest value to the optical industry of Germany. It would be quite unwise of him, as a servant of the only firm of optical glass makers in this country, to go deeply into the question; were he to do so it might be construed into an attempt to secure an advertisement; but their former President, Dr. Rosenhain and many others, could bear him out in saying that they had conducted experiments running into thousands whereby at least a thousand types of glass had been made, and in many cases, although those glasses had been kept from year to year, they had ultimately found a resting-place on the scrapheap because they had not been applied. The greatest thing needed in this country, he thought, was not a place for the manufacture of optical glass, but a place where it could be ascertained exactly what type of glass should be used for a specific purpose. It was the co-operation between the makers and the users of optical glass that would ultimately raise the optical industry of this country to the level of the German house.

MAJOR WILLIAMS pointed out that since the war there had been an enormous increase in the military requirements in connection with optical instruments. When they remembered that the Army had been increased to the extent of something like a million men, six or seven per cent. of whom would want field glasses, and one-fourth would want telescopes of different sorts, both for sighting guns and for observation; that every hundred men would need that very expen-

sive instrument, a range-finder—one of the most accurately-made instruments, capable of measuring up to 6,000 yards with an error of only two or three per cent.; when they remembered the enormous numbers of surveying instruments and of heliographs with their mirrors accurately made and silvered, they could form some conception of the amount of work which the Government required to be done at the present time. He imagined that most optical firms who actually made those articles were fairly busy now, and would be for some months, if not till the end of the war.

DR. WALMSLEY said in connection with the production of optical glass they must all be struck by one or two salient facts. One of those salient facts was that in the whole world there were only three firms manufacturing optical glass, and if it were true that the Jena firm was the only one which had successfully attempted to make optical glass, they had to ask themselves the question, why was that so? He thought the answer to that question lay at the root of most of the evils. They were practically assured that the making of optical glass was not a commercially successful proposition; yet in Jena and in Paris it paid. He thought the fact that there were only those three firms showed there was a great deal underlying the question. If it were a matter of cent. per cent. profit they would not be dependent upon three firms for the whole supply of optical glass for scientific purposes. Mr. Lamb had told them that it cost £100 to make an experiment in the manufacture of optical glass, and an experiment might very well fail as often as, or oftener than, it succeeded. It was not a commercial proposition, therefore, to make optical glass experiments. Now the firm of Schott was a glass-making firm, but it was allied with instrument-making firms who used its products as glass-makers, and notwithstanding the figures which had just been given by Major Williams, he thought they might draw the conclu-

sion that the demand for optical glass was not such as to produce supplies under the present conditions of manufacture. The difficulties which stood in the way were set by Nature. Research might help them; research would help them and ought to help them, but it could not alter the fundamental conditions which made it so difficult to repeat a second time, even with all the knowledge gained by the previous melting, a glass exactly similar in all respects to the first. It could not be certainly done, and there was the kernel of the whole matter. Then they came to the uses of the glass, and it seemed to him the question must be viewed in this way. They could not be absolutely sure of successfully melting a particular kind of glass, which was wanted in very small quantities, but was wanted very badly, and must be had in those small quantities, otherwise the design of the instruments must be altered. It seemed to him that if it were a fundamental condition, which he hoped research might eventually remove, that a particular kind of glass could not be repeated with absolute certainty, then they were thrown back upon having ready to use the glass so produced a corps of designers who could make the necessary modifications in the instrument without impairing its final efficiency for the particular purpose for which it was intended, and that, if he might say so, was where he came in, and where Mr. Chalmers came in. They wanted technical education—not the academical education upon which Mr. Blakesley had very rightly poured the vials of his scorn, but really good technical education. The teaching in the textbooks was sometimes wrong. In this respect the optical industry was now in a very similar position to that which existed in the electrical trade somewhere about the year 1860, when the electric telegraph had not been long introduced. In the growing development of telegraphy, the laying of the first Atlantic cable and so on, the schools and universities, the teachers of elec-

tricity, were left behind by the manufacturing laboratories, who had in existence commercial standards for which there were at that time no corresponding scientific standards, and a British Association Committee sat for many years in order to produce such standards. To-day, it seemed to him, they were in much the same position with regard to optics; the optics of the manufacturers' testing-room and designing-room were far in advance of the optics of the university. Now that was a topsy-turvy position, and one which ought not to exist. The university should lead in such matters, and he was afraid it did not. To his mind one of the most essential steps to be taken was the establishment of a sound school of technical optics. (Hear, hear.) The figures Major Williams had given them were simply staggering; they wanted 10,000 range-finders, 500,000 telescopes, and goodness only knew how many thousand field glasses. This country could not produce them; it had not got the trained workmen to begin with, and it had not got the trained designers to take advantage of any glass which was within the limits of possibility of use which might be supplied to the instrument-makers. He thought the first essential was for them to attend to optical education. Unfortunately education was a thing which could not be improvised in a week; its results could not be ready within the next fortnight. They had lost at any rate ten years of valuable time on that very matter, and that was why they were in such a muddle to-day. Those figures of Major Williams were quite enough to convince everyone of the hopeless position the country was in now. Incidentally he might mention, as bearing on that subject, that the classes at the Northampton Institute were at the present time simply denuded of their students. Why? They were all working overtime, and therefore could not come to the classes. And now was the very time they ought to be attending the classes, in order to be

trained for the needs of the future. He had addressed himself merely to two aspects of the question, and would leave it to others better qualified than himself to deal with other branches of the subject.

MR. M. BENTZON said that he hoped he would not be treading on anyone's toes; or be thought guilty of introducing an unduly delicate topic, in expressing his strong opinion that the real origin of the whole evil was undoubtedly free trade. They were short of field glasses and of every kind of instrument required by the Army and Navy, and now found themselves compelled to allow a German firm in England to manufacture these articles. He considered it a perfect disgrace to the nation that such a state of things—due to unfair free trade—should be possible. The reason that they as a nation had not succeeded as manufacturers, was as simple as A, B, C. Obviously it was possible to manufacture more cheaply for 110 millions of people than for 40 millions, which was roughly the population of these islands. Under fair conditions England would have no difficulty to compete, but if she now attempted to export any of these articles to Germany or to America, or to any other country, they were immediately stopped by high tariff wall, while on the other hand those countries had protection for the home trade and for their export trade as well. How, Mr. Bentzon then asked, could British trade prosper under those unfair conditions? They might do what they liked with regard to improved technical education, but they could never succeed under present conditions because manufacturers here had not the ghost of a chance of producing at a sufficiently low price to compete with a country which had an open market for at least three times the quantity. In his opinion that was the whole case in a nutshell. Now as to optical glass. The reason they could not produce optical glass in sufficient variety and quantity was chiefly on account of the costly experiments. It

would not pay anyone to start a manufactory solely and purely for optical glass. There must be something else to keep the pot boiling. Well, the three firms mentioned had something else to sell; they manufactured very large quantities of spectacle glass out of which a fair profit was made, and that profit helped them to go ahead and spend money on the experiments necessary to produce new kinds of glass. If protection could be secured in this country when the war was over, he had not the slightest doubt that British manufacturers would be able to produce both optical glass and spectacle glass on a sufficiently large scale to meet all the requirements of the trade and that glass "made in Germany" could be dispensed with in England as well as in the Colonies.

MR. W. A. DIXEY said he could not speak as an expert, because he knew nothing specially about the production of optical glass; but he could give his experiences as a trader who had been mixed up with optical glass more or less all his life. One thing that struck him was that there was a sort of an atmosphere of mystery about the production of optical glass. (Laughter.) They had had a good deal of it that night, and it had more or less fogged the discussion. Speaking as a non-expert, he simply did not believe half they heard about the difficulties of production. One of the points raised was the enormous expense of experiments in the production of glass. Now he did not know whether there was any foundation for what he was going to tell them; he gave it as a pure bit of gossip and nothing else; but when he was in Germany in 1877, a few years after Schott had made his experiments, the story that went round the technical school where he (the speaker) was, in Germany, was that Schott had been provided by the Government with the fabulous amount of 6,000 marks (£300) a year for three years—(laughter)—with the result that he was able to solve the question of producing optical glass, and to

start his works. Well, £900 was not a very large sum to expend, and he could quite imagine that if there were any prospect of getting anything like Schott's results over here the £900 would be easily forthcoming. He repeated that he had no authority but gossip for that story, and perhaps a rather faulty recollection. He thought the main difficulty in the instrument trade in England (and they felt it particularly just now because of the great and sudden demand for optical instruments for military purposes) was this question of optical glass. He would give them a specific case. He knew of a firm of prism binocular makers who had accepted a contract from the Government—a moderate contract with an extended time for delivery. If they could have produced them they could have multiplied the quantity of that contract by 10, and very much reduced the time. But they were held up. Why? Because they could not get the glass for the prisms. It was a little difficult to speak upon the subject in view of the people who were present there, but he was told that their one English firm was impossible—he supposed because they were under contract to produce other things, and it was impossible to get the glass from them. The firm he was speaking of were dependent upon France for their prism glass, and there was very great difficulty in getting prism glass in France now. With regard to the question of skilled workmen, which Dr. Walmsley had mentioned, that was not such a real difficulty, to his mind. He knew of very many workmen who could do all the mechanical work necessary for the production of prism binoculars; they were now working in other directions, but could easily be turned on to that work and would very soon become adept at it. But as far as he could judge from his experience (and he was a sort of half-expert on this subject) the main question was the difficulty of procuring the optical glass. He would like to know whether Mr. Blakesley could tell them any-

thing more about the British Science Guild. He had been interested to receive a communication from that Guild in which he was asked to supply information with regard to the production of optical glass and so on, and the difficulties in connection therewith. He had responded to the best of his ability, and he would be very glad to hear from Mr. Blakesley what practical measures that body of gentlemen proposed to take or were taking.

MR. CHALMERS said he had had occasion recently to go into the question of the composition of optical glass, and into the history of its developments, and in doing so he had come across some very curious facts. The first of these curious facts was that the first notable success in the production of optical glass was not achieved in either of the three countries which had been mentioned in the discussion, but in Switzerland. But before Guinand could make any practical success in glass-making for the purpose of optical instruments he had the co-operation of Fraunhofer and Utschneider, who were really an instrument-making firm, and his subsequent success was due entirely to the fact that they were capable of appreciating when he had got good results; it was the co-operation of the instrument-maker and the glass-maker that made that firm's success. It was curious to note that no one present had seemed to realise that the factory in Switzerland was still in existence, or had been until quite recently. In that case it was the fact that the profit was not made on the glass-making as such, but the profit made on the manufacture of instruments, for the firms allied with the glass factory kept the latter going. Turning to France, they found it once more the case that it was in association with instrument-makers that the developments in glass making had taken place. Then, when they came to Chance Brothers' works in England, the reason of their success was nothing to do with optical instruments, but was closely associ-

ated with the fact that they were manufacturers of lighthouse apparatus. They were bound to have good glass for that apparatus, and though of course they did not use the same quality of glass for that as for optical instruments, yet the skill and knowledge they acquired in connection with their optical glass experiments had, he believed, laid the foundation of their success as manufacturers of lighthouse apparatus. Then the development of the Schott and Abbe works was also very curious. The reason for the establishment of the Jena works was that Abbe found that in order to improve his microscope objectives it was necessary to make glass of different properties from those of any of the glasses then in use. The whole of that experimental work was undertaken primarily because Abbe desired to improve those microscope objectives. It was not only the money of the Prussian Government which made those works; it was the fact that Abbe was prepared to devote his own ability and spend his own money for their development. It was in fact to Abbe's own personal devotion to science that those works owed their success. The figure quoted by Mr. Dixey was, he believed, perfectly correct as referring to certain experiments on a small scale which Schott and Abbe undertook; they did laboratory experiments for some years, the cost of which was exceedingly small. But when the question came up of putting the results of those laboratory experiments into practice on a very much larger scale, a much larger subsidy was granted—he believed £20,000; and that was the money which went to pay for those £50 and £100 experiments which had been referred to. And not only was the Prussian Government's money put into that work, but also to a very large extent the private money of Abbe. But Abbe was recompensed, and Schott was recompensed, not by the profits on the optical glass work, but by the fact that the Zeiss firm in Jena, using that optical glass, were able to make very large profits by making

new types of instruments, and it was from the ultimate results of that instrument-manufacturing work that the profits came. Moreover, they had learnt by their experiments something about glass which other people did not know, and the consequence was that for a long time at any rate, they were able to make the best lamp chimneys, and that was another source from which their profits came. Those were the reasons why it had been possible to incur those expenses without any hope of profit, and why it was so exceedingly difficult for any other firm to compete, because optical-glass making could not be made to pay its way, and the only profits were incidental ones. That was the reason why the business had been confined to so few people. There was one point he would like to call Mr. Blakesley's attention to, and that was the fact that he had ignored altogether the striking success of the firm of Mantois, who had been able to reproduce most of the difficult Schott glass in recent years. He would say this for Messrs. Chance, that although they had not attempted to make the more difficult glasses, they had concentrated their attention upon improving the quality of the more common types of optical glasses, and in many cases they had made these better than their competitors had been able to do. The great difficulty which had stood in the way of the employment of English glass in the past had been the fact that lenses had been designed for manufacture with the Schott or the Mantois glasses, and when a manufacturer had designed a lens for one particular kind of glass, and had perhaps spent six months in perfecting that lens, he was not likely to change for some other glass unless it could be used for the same design of lens. Consequently, in many cases, a lens designed in connection with one firm's glass would be made with that firm's glass, although it might be in reality inferior to another, because it saved the expense of re-calculating and re-designing, and one reason that the English firm

had not done so well as its competitors had been that they had not had associated with them the same amount of optical knowledge as to how their glass could best be used in practical work; there had not been in Birmingham a first-class optical firm using all varieties of glass, and capable of taking advantage of the special glasses of that firm, making their own special designs and using the different glasses for those designs in the same way that the Zeiss firm used the Schott glasses. When a calculation was made and a design produced, very often manufacturers were unwilling to go to the expense or trouble of getting equally good results by the use of the glasses which were available in England, and the consequences had been that as the business had grown, the number of glasses available to the manufacturer abroad had increased; and had diminished in comparison in England. Therefore if one wished to design a lens with a good chance of success, one would go to the firm which supplied the greater choice of glasses, because experience taught that it was very often necessary to substitute a slightly different glass from that used in the calculations. There was one special defect of the lens system which could not be eliminated, and quite at the end of a long series of calculations it was found necessary to slightly modify the type of glass used. There was such a thing as reputation to be considered in the matter; and he was afraid that within the last ten or twenty years full credit had not been given to England for the work her manufacturers had been doing. Mr. Blakesley had mentioned the question of optical surfaces, and had spoken as if England could not make as good optical surfaces as were made abroad. Now it had been recognised throughout the last ten years at any rate that the best optical work in the world had been made in England. He was speaking of actual optical surface work, and his evidence for that statement was that when it came to producing the best parallel plate of glass

ever required by the Zeiss firm, the work was ordered from London. He thought that was evidence that when a piece of work of the highest possible standard of perfection that it was possible to obtain was required, the order was placed here. Also it must not be forgotten that when it came to the question of selecting the best big telescope objective made of recent years, the order came to an English firm which at the present time supplied practically all the best quality of telescope objectives. He would only mention those two cases, of which he had absolute personal knowledge. He thought, therefore, it would be admitted that in certain departments of optical glass work England had more than held her own, and he could say from his own experience that there had been in this country during the last ten years an advance in regard to optical work which one would not have believed without actually seeing it. Those of them who had been able to see the optical work exhibited at the two Optical Conventions held in recent years must have been absolutely astounded at the big developments which had taken place. It was not right to say that foreign work was always better than their own, and he thought people ought to examine the work and compare it without prejudice. If they did that they would find in a larger number of cases that the English work compared very favourably with that done abroad. And now he was going to make a very serious criticism with regard to the English optician. A great many of the present difficulties in connection with the optical trade in this country were due to the fact that every optical instrument maker would insist on making everything. Not content with making what he could make well, he would take an order for anything. No well-organised manufactory in other industries attempted to do anything and everything, but when it came to the question of optical instruments a manufacturer capable of doing the finest possible work would accept

orders for the very cheapest, and the consequences were very often disastrous. Optical firms ought in his opinion to concentrate more upon the type of instrument for which they were best fitted. At the present time in this country there were four firms all making microscopes, and not content with making microscopes for one particular purpose—not content that one firm should make one type as well as it could be made, and another firm another type, each firm would insist on making every little part and every little accessory for every description of microscope; instead of making one thing and making it well, they would compete with each other for every possible order they could get. He thought it would be very desirable for those having the power to place orders with optical firms to as far as possible place orders for certain specialities with a particular firm, or, at any rate, to distribute their orders between a limited number of firms for particular specialities, and thus try and assist the development of particular types of instruments in particular factories. When he was in Jena a year or two ago, he was discussing with one of the members of the firm the making of a certain piece of apparatus. He asked, "Why are you not making that now?" And the answer was that they got orders for so few that it was not thought worth while making them. The member of the firm he was talking to had said, "I have not within the last year passed into my branch of the factory any order for less than a gross." And that was in connection with scientific instruments, orders for which in the ordinary way were reckoned by ones and twos. That showed how the organisation of that firm was directed to turning things out in quantities, and to improving the apparatus by concentrating the work in special factories. At the same time he had visited another German firm closely associated with the Zeiss firm, and saw something which had been designed in Jena, and which he knew used to be

made by Zeiss. On asking the reason, he was told, "Oh, we make that now, and they make so and so, which we used to make." And the result was they were able to make both articles more cheaply and in larger quantities. He thought that was where opticians in England had failed; they had tried to make everything that was possible to be made in the optical world, and the consequence was there was no profit in them either for themselves or for anybody else.

MR. T. Y. BAKER said there was one point on which none of the previous speakers had touched, relating to the immediate future of the optical industry. We were at present engaged in war, and there must be for the two fighting services a very great number of optical problems that might suitably be settled by the experts belonging to the Optical Society. He merely put it forward as a suggestion that the Society should communicate to the authorities (if possible in conjunction with other scientific societies in the metropolis) their willingness to assist in any possible way to solve such naval and military problems as the War Office and Admiralty might have at present to face.

The Author's Reply.

MR. BLAKESLEY, in replying on the discussion, said he was sorry he could not supply the information asked for by Mr. Dixey with regard to the British Science Guild, because he had been unfortunately prevented by other duties from attending the meetings of the Committee of which he was a member. He believed, however, that the matter was receiving attention, and hoped that a report would be issued before very long.

DR. WALMSLEY said that as Secretary of the Guild he was in a position to state that the report was at the present time in preparation.

MR. BLAKESLEY, continuing, said Mr. Lamb appeared to be under the impression that he

(the speaker) had neglected to mention the name of Abbe as one of the causes of the success of the German firm. He could only conclude that Mr. Lamb had not quite heard his remarks on this point, because as a matter of fact, he had drawn an analogy between the co-operation which had existed between Fraunhofer and Utschneider on the one hand and Schott and Abbe on the other. Mr. Chalmers had, he thought, misapprehended him in regard to another matter. He had not criticised the execution of the spherical and plane surfaces in England at all, nor anywhere else; what he had said was that he believed in the future there was room for the possibility of the execution of surfaces which were not attempted now, except empirically. He believed it was the case that spherical surfaces were specified in almost every instance of a telescope or microscope lens, and then some empirical modification was decided upon by trial. What he said was that these matters might be made the subject of calculation, and the shape obtained at once, which would not necessarily be spherical. He regretted not to have mentioned the Mantois firm, which was a firm for which he had the highest possible regard. He knew some of their glasses and had used them, and it had certainly not been his intention to pass them over without remark. It was a very remarkable firm, and his only complaint with regard to them was that they did not describe in their catalogues the peculiarities of their glasses as fully as was done by the German firm; that they were liable to split, liable to have bubbles, and so forth. Those were extremely useful bits of information. Dr. Walmsley had said it was hardly a commercial matter, the making of new optical glasses. That was quite true, nor was it judicious to undertake to make such glass if you had not first taken the necessary experiments in hand. But after the experimental work was over, he fancied that the manufacture of such glass was an extremely profitable affair, when it turned out to be use-

ful for some design of instrument for which there was a large demand. The question of military problems had been touched upon by Major Williams. No doubt the chief military problem of the present day was the multiplication of field glasses, telescopes, and so on, but the problem they were now contemplating was, of course, something quite different from that sort of thing. In conclusion, he apologised for the necessarily brief and imperfect manner in which he had been compelled to deal with this very large question, owing to his time having been so much occupied in other directions.

The PRESIDENT said they had all enjoyed the most interesting and, he hoped, helpful and stimulating discussion arising out of Mr. Blakesley's very able paper. A report of what had been said would appear in the official organ of the Society, and he trusted those who were deeply concerned in these problems would read that report and consider the points raised very carefully. Were he himself to contribute anything to the discussion, it must necessarily be from the point of view of one who, to adopt Mr. Dixey's modest description of himself, knew nothing at all about the matter. (Laughter.) It seemed to him as an onlooker that we had to some extent been following false gods in this country. First of all they had been suffering for the last 25 years from a bad wave of national melancholia, and that had been heartily endorsed, as regarded its truth, by Teuton influences. They had offered the incense of worship to German intellect, and Germany had accepted that as only its due; so that it had looked down upon poor England and said of all its work, "Can any good come out of Nazareth?" The British public was largely to blame, for an English worker who discovered a new planet or a new element, if either were now possible, was less esteemed than if he had won a prize fight. English students who were aiming at ordinary qualifications were worshipping the diploma

fetish too much. If they were taught anything they asked, "Shall I be asked that in the examination?" They cared precious little for the vital value of the fact taught, as to the influence it might have in spreading a new conception; they said, "Shall I want it for my examination?" And when they had got that piece of coloured paper—that "scrap of paper"—(laughter)—they said, "We are qualified," and regarded themselves as having no more to learn. And in the technical institutes what did they find? Crowds of elementary classes, and teachers who were eminently qualified to teach real knowledge engaged in cramming perpetually elementary principles; they had no time to go further, because they had to take the next class, and so they had to go on teaching first principles again and again and again, their real talent being unappreciated and largely wasted. Another point he had observed was the displacement of English effort owing to causes upon which Mr. Bentzon had dwelt so strongly. One man he knew of turned out very excellent microscopes, but few in number, and he gave up making them because he was able to sell microscopes by Leitz and Zeiss, and other makers, at a greater profit to himself, so he said, "Why should I bother about turning out instruments, having the Workmen's Compensation Act always hanging over my head, and the Factory Acts always harassing me, and all sorts of troubles with my workmen, when the German comes in here and says, 'Here you are; unpack my goods, stick them on your shelf, pass them over the counter, and make far more profit than you can possibly make from your own'?" What was the result? Why, of course, that the native industry was bound to be killed. That was one of the difficulties they had to encounter. But he would not like to have the discussion end up on a note of melancholia. When they looked back on the glorious past of British science, they found that almost every fundamental, overwhelming dis-

covery which had revolutionised the work of science had come from British inspiration. He did not refer to the smaller discoveries, which often, however, led to very great results, as in the case of Abbe's work, but those which went far beyond, and stood upon a far higher pinnacle—where some genius had discovered some new principle and generated some revolutionising idea. If they looked at the past history of Germany as regarded its work in applied science during the last fifty years, it would be found that they had hardly done any single thing of highest rank; they had done many of lower rank but hardly anything of the first order. It seemed as if the foundation of the German Empire had brought the stifling hand of Prussia over the enlightenment of the German mind, and reduced it to that state of discipline which enabled it to do splendid secondary work but stifled the ability to do great original work which was displayed by other nations. He could only think on the spur of the moment of one great discovery of the first rank which had emanated from Germany, namely the x -rays, and after all, Röntgen, he believed, was not a German, but a Dutchman.

He thought all would admit that the Germans had excelled in this—that they had seen the commercial value of science much more than our own people had. The British manufacturer, in regard to scientific advice, was apt to go along without any at all until he came to a point where he was absolutely compelled to call in scientific assistance, and then he went to the man of the biggest reputation. Now the man with the biggest reputation was usually far and away the busiest man, and wanted an exorbitant fee for his services if he was going to be taken away from his ordinary money-earning activities; and the result was that the manufacturer found scientific co-operation far too expensive. The Germans, on the other hand, were far more prone to employ

the services of enthusiastic young men, who were willing to work for work's sake, whose financial responsibilities were less, and who were willing and glad to give their whole services for a modest salary. The result was that German workshops were able to enjoy much more scientific co-operation than was the case in England. He had perhaps said as much as was fair for a mere bystander to say, and he would conclude by formally conveying to Mr. Blakesley their sincere thanks for the great trouble he had taken.

ON OPHTHALMOLOGICAL INSTRUMENT
DESIGN, WITH SPECIAL REFERENCE
TO ACCURACY AND RAPIDITY OF
WORKING.

BY WILLIAM ETTLES, M.D., M.S., F.R.C.S.

*Presidential Address delivered 10th December,
1914.*

THE usual plan adopted by presidents of learned societies is to place before their audience a brief resumé of the history, development, and wide-reaching importance of that subject, for the further study of which the individual society has been founded. When delivered by a man of great scientific distinction, the discourse is usually fascinating by reason of the great breadth and depth of knowledge possessed by the exponent. Sometimes, however, as in my case, the president does not possess such qualifications, and then a laboured exposition of historical progress becomes rather a bore to an audience.

In these days, we are inclined to regard tradition as being less binding than in former times, and if a president cannot conform to the older rule with advantage to the society, it is much better that he should make his address on a subject on which he had worked for a long time, and so bring at least the cachet of intimate acquaintance to the consideration of certain special points, which he trusts will interest most of the members.

If, therefore, my remarks to-night savour much more of a simple contribution than of the formal address, I should be glad if you, on your part, would take it in that light; and instead

of relieving me of the exposure to criticism and discussion, which is usually observed on such occasions, I should be very glad if the members present would feel free to raise any points which strike them as needing amplification or criticism.

I am approaching this subject from the point of view of the ophthalmic surgeon engaged in a busy practice, to whom expense of material is a matter of no consideration whatever, if by any outlay he can secure greater efficiency and speed in his work. It is true that a "bad workman quarrels with his tools," but it does not follow that a good workman can work with bad-tools. Indeed, we know that to get the highest quality of work we must give the workman the finest tools obtainable, since none other will satisfy his demands.

There is probably no branch of medical work which requires a more thorough and searching examination than does ophthalmology. The history of the case has to be listened to, and the external and internal appearance of the eye to be inspected, the patient's general constitution, occupations, habits, have to be borne in mind, tension has to be tested, and the refraction to be carefully measured, exploration of the fundus by the ophthalmoscope carried out, the field of vision recorded as well as the muscular balance of the eyes, and all has to be done within thirty minutes. Every minute is of value, and to save ten is to save one-third of the consultation, and is to be able to give mature instead of hurried consideration to the crux of the case which examination has revealed.

I know that it is not enough to provide an oculist with time-saving tools: the minutes may be wasted by the man who has not disciplined his mind, for the consulting-room chair had many interesting occupants. Most people who are engaged in business deal with those in the same line of work or in closely allied branches; but the oculist deals with everybody whose

eyes have become defective, and so cabinet ministers, generals, admirals, solicitors, working men, seamstresses, come one after the other, and the temptation to glean facts as to the movements of the outside world from these many sources, while he himself is chained up in the consulting-room, is a very great temptation. But it is one which must be strenuously resisted in the patient's interest, for the time is all too short for the consideration of the many ocular problems.

We will assume that that lesson has been learnt, and what is wanted now is accuracy, for accuracy is economy of time. To measure and note carefully, and to know the measurement is exact, makes for rapidity. Slack measurement which demands repetition is one of the truest ways of wasting minutes.

I think that at the present time we are too dependent on the workman's ideas. Trial-frames, ophthalmoscopes, lenses, and all the other means at our hand are made by workmen who have never seen a case under examination, and have never handled the instruments in practice. The apparatus is obtained by the professional man who has, at the outset of his career when he purchases his equipment, little experience of what is required. So it comes to this, that the workman introduces a modification which in his view is a good one, and he takes it to his employer, who says, "Yes, that looks very nice; make me a hundred of these, and we will put them in the window"; and this modification is bought by one who does not really know whether it is practically valuable, till long experience has made him feel the faults of the instrument, and perhaps reconciled him to them.

I am approaching this series of observations then, from the point of view of one who has used his apparatus for twenty-five years, and is now about to lay in a fresh stock of tools, knowing very decidedly what he wants, and is com-

ing to you to know whether you can supply them.

Patient's Chair.

It might seem superfluous to say much about a chair on which the patient is to be seated, but in point of fact it does require some consideration.

I do not know if you have ever seen an ophthalmic surgeon doing a retinoscopy on a little child who has been placed under atropine. You would find that he has to develop a marked lateral curvature of the spine in his efforts to bring his head on a level with the patient's; and when he is standing up to do the subjective testing, the strain on his back is indicated by an occasional straighten-up with a deep sigh.

The ideal thing would be to have the patient's head on the same level as the operator's during the sitting examinations. We should therefore so arrange matters that the little patient is lifted to a position where his eyes will occupy the horizontal plane of the adult.

The seat of an ordinary chair is 17 inches from the ground. If it is an armchair, as it should be, the height of the arms from the ground is 36 inches, while the average height of the adult's eyes from the ground level is 50 inches. If a small padded board were laid across the arms of the armchair, and the child placed upon that, it would be found that he had a quite comfortable and secure seat; and the operator is now no longer obliged to undergo the long continued contortion.

The patients' chair ought to be a rotating one. By doing that we can have heavy apparatus, such as the perimeter and the ophthalmometer, on either side of him, well out of the way, and by simply giving the seat a twist round one side or the other, he is placed opposite that apparatus.

Elaborate rising and falling chairs, somewhat on the principle of dental chairs, have



FIG. 1.

P, 1, T. = Subj. Testing of R.E.

P, 2, T. = Subj. Testing of L.E.

P, H, 5 = Perimetry.

P, 3, T. = Retinoscopy.

P, K, 4 = Ophthalmometry.

been devised; but in practice they are not so good as the padded-board seat for children.

Lamp.

For focal illumination for ophthalmoscopy, retinoscopy and other manipulations concerned with examination, we require an electric lamp, which can be moved quickly into any position



FIG. 2.

within a given perimeter of the patient's head. Numbers of lamps have been devised for this purpose, some of them running on friction rollers up and down a vertical rod. They work all right at first, but after a time one finds that they either stick, or they get loose; in that event they run down and strain the cord, and break the filament of a focus lamp.

Much the best is the balanced ball and socket lamp known as "Lea's Patent" lamp, made by Messrs. Player & Mitchell, Birmingham. This lamp was, I believe, designed for the engineering laboratories of the Mason College, the intention being that it could be easily and quickly moved anywhere over the large surface of a drawing board.

I have used this lamp in my practice now for the last ten years, and I regard it as



FIG. 3.

superior to any other. The movements are effected with a touch of the fingers, exactly and silently, and the lamp stops instantly in any position in which it is placed. The range of movement, too, is much greater than with any other design.

Table.

The table on which the trial case is placed should be a flat one, and the case itself should be the ordinary tray. All rolled-top desk types

are quite useless for the ophthalmic surgeon's work.

The table should have four slender, rather tall legs, unencumbered with any sort of fitting between. The idea is that it can be placed in front of a patient in the same way as an invalid bed-table. It should also be furnished with handles running along the sides; and, above all, with a little writing pad—such as one uses for telephone messages—screwed to it. It is an advantage to have a small stool on either side of the patient's chair, so that the operator can sit down.

I draw a plan shewing the position occupied by the table when doing each eye, and when doing retinoscopy. By scribbling down the visual acuity and the results of lenses on the pad, one has not to be constantly rising from one's seat to make notes in the case book. When the examination is completed, the paper can be torn off, and the notes then entered.

Test Types.

The ordinary Snellen test types are those generally in use, and no doubt they serve their purpose very well, but those who have worked with them extensively feel that the series is too coarse.

It would take too long to go into the whole matter of test types, but I would briefly draw your attention to a chart which I designed. Practically, on the left hand side, it represents the old one, and on the right hand side, the new series. The chart has to be read downwards in a zig-zag manner, and we get a series of $1/10$, $1/8$, $1/6$, $1/5$, $1/4$, $1/3$, $1/2$, $2/3$, $4/5$, 1. The old series, as you know, is $1/10$, $1/6$, $1/4$, $1/3$, $1/2$, $2/3$, 1.

Thus if we use the old board, a patient may have vision = $6/60$, that is $1/10$, or 0.1. Now, after some treatment, the examination might be repeated, and vision still found to be the same, because the patient could not read $6/36$.

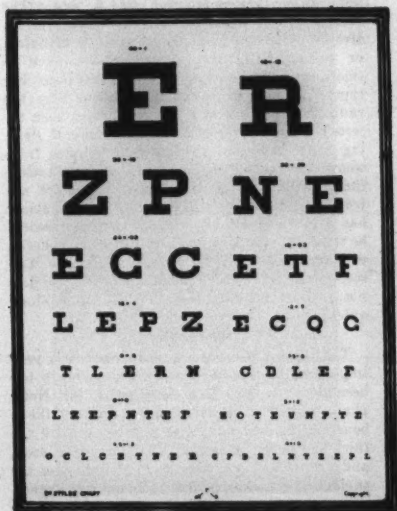


Fig 4.

But in the new test it might be found that vision had risen from $1/10$ to $1/8$, that is $6/60$ to $6/48$, and one would get an idea which way the case was going; whereas on the old chart one would have no idea at all.

You will observe that the test types go beyond $6/6$: they go to $6/5$, $6/4.5$, $6/4$. The reason is that in testing low degrees of refractive error it may be taken as a kind of axiom or golden rule that "low degrees behave to small types as coarse degrees behave to big types." That is one of the reasons why the radiating lines, or any fan tests, have been rejected from ophthalmic clinics. One is dealing with so many unintelligent people, from whom one wants "yes" or "no" answers; while the clearness of the lines on a fan chart are frequently the subject of opinion. If a patient has a low degree of error, $6/4$ cannot possibly be seen. One puts up weak cylinders or spheres, or both, and there is an improvement. You see, the patient either reads these letters, or does not read them, and one can thus get along quickly.

Illumination.

The illumination of the test chart is a very important point. As you all know, much has been done by Mr. Mackinney with the Holophane reflector. It is a mirror shaped like a housemaid's dust-pan, with a tube lamp in the thick end of it. It acts as a cylindrical mirror, and throws a pretty even flood of light over the surface of the chart. But it is not my experience that that evenness is anything like what is claimed for it. It tails off very much just where we want it mostly, namely, in the lower part where the fine letters are; and that, of course, must be the case, because we cannot get past the physical law of the ratio of intensity to distance.

I find a very good plan is to put a piece of mirror underneath the test types. It gives one the effect of a second reflector, and gives the

required added illumination to the lower portion.

Of late a good deal has been done in the way of designing tests which expose only *one line of letters at a time*. The most practical one is that of Mr. Dixey's, which I show you. I have used it, and used it pretty roughly and



FIG. 5.

continuously for some years now, and it never gives the slightest trouble. The cards drop down in a pleasant way, and we quite get over the difficulty of illumination, as the area to be lighted is so small.

There are subsidiary advantages about the system of exposing one line, because the patient, and the patient's relatives, are not continu-

ally expecting that vision should be raised to a supranormal extent. One may have, for instance, a case of conical cornea in which optical miracles have been done in the way of increasing visual acuity, and you will find that the patient's attention is riveted, not on what has been done, but on what has not yet been done. Now, the drop lines are very useful because the patient, and the patient's relatives, cannot see what there is beyond what has been gained.

Another most important point is that the attention is fixed on what is required. You can imagine the feelings of one pressed for time who asks a patient to read the letters of 6/9 and who finds that he always begins with the top letter, and then quarrels about whether the fourth or fifth line is really that, because he does not regard the top letter as a line at all!

Trial Case.

I assume that the trial case is familiar to all of you. It does not seem to have altered sensibly since it first came into use a generation or two ago; and that must be to some extent a testimonial to its fitness, otherwise one might assume that important modifications would have been introduced. The trial case is somewhat comparable to a wife. One becomes associated with it for better or for worse before one has had much experience of what it entails; and by the time a man has acquired sufficient clinical skill to be able to criticise its arrangements, he has become more or less wedded to it, and waives these points.

THE SUFFICIENT SERIES.

I think one of the features that would first strike a beginner in looking at the trial case of a man engaged in a busy practice, would be first its dirtiness, and, secondly, the frequency with which the lenses were in their wrong places. Somehow or another one does

not mind that so much as the *lack of a sufficient series*. When the trial case was introduced errors of refraction did not by any means receive the close attention that they do to-day; and although our views have altered, our means have remained stationary. Take, for instance, the concave series at its upper end. We find such a series as half a diopter up to 8, a whole diopter up to 12, and then steps of 2 D. from 12 to 20. Now, that is quite insufficient. Myopia of 13 D., for instance, is an extremely common amount, and yet very few, if any, trial cases have a -13 D. lens. The same with, say, 15 D., it is not a good plan to have to place a -14 D.S. in the back cell, and a -1 D.S. in front of it; for these cases are nearly always associated with some amount of astigmatism, and at least we want to know whether that astigmatism has much significance or not. If we have a three-cell trial frame the loss of light in the internal reflection of six surfaces is a great annoyance, which ought to be eliminated, and can be eliminated, by having a proper series.

In my belief, the day is quite gone past when the trial case of the ordinary pattern can be regarded as meeting modern needs; and everyone who wishes to invest in one and work it seriously should insist on having at least one-diopter intervals from 15 to 22. I say 22 deliberately, because one passes the 20 D. limit quite frequently, and there is no reason at all why our tools should not be beyond the compass of our needs.

It is not to be assumed that the same holds good with regard to the convex series, for this reason, that while high myopia is quite common, high hypermetropia is exceedingly rare. The convex series, in the higher powers, are almost entirely reserved for cataract cases; cases in which the crystalline lens has been removed, and where the supplementary lens to be worn in front of the eye is about 10 D. for distance, and about 14 D. for near work. The 10 to 14 D. series is

in frequent requirement, and above 14 we seldom need the lenses. Again, below 10, from 10 to 5, we seldom need them. Most hypermetropes are under 5 D., unless they are aphakics. On the convex side, therefore, it is not by any means necessary to have the fine series.

The next point I will touch on is that continuous trouble which everyone has in the *difference between the actual and the numerical powers*. We know that with the higher series of lenses, owing to their thickness, refractive index, and the question as to whether they are bi-spherical, or plano-spherical, or meniscus, each trial case seems to be a kind of unit in itself; and every wholesale manufacturer will agree feelingly with me, when I say that most of the lenses returned as erroneous are so because the manufacturer works to one neutralising power, and the prescriber to another.

PLANO-SPHERICAL TRIAL LENSES.

But to go into this question at length would demand much more time than is available for this evening, and I must content myself by just raising the point, and asking whether the trial lenses ought not to be plano-spherical! It seems to me that that would eliminate the difference, and would always make them neutralisable.

A very important point also arises in connection with the *thickness of the lenses*. Surely there is no reason whatever why they should be so very thick and weighty. After all, the width of the cornea is only 10 mm., and the lens itself is practically four times that. This arose in the old days before trial frames had adjustable centres. They were simply rigid frames, like a pair of spectacles. It was necessary, then, to have large diameter lenses, so as to allow for varying sizes of face. But those days have gone past, and it is highly unnecessary to keep that up.

Of course, we all know that the smaller the diameter of the lens, the less is its weight and thickness. Of late, a very nice series has become

available, an instance of which I show you here. (Sample shown.)

The Uni-Bifocal Lens Co. is engaged in making them out of solid glass. They have ample superficies for working, and at the same time the highest powers are no thicker than the low ones.

The makers have provided us with these thin lenses, but not with suitable trial frames. The cells are made of the old breadth to allow for the combination of a thick spherical lens with a thick cylinder. In these spaces the new lenses are placed, and the lenses are just as separated, and just as upset, as to the nodal points, as if they were thick lenses. They are clearly wrong, and we ought to have our trial frames with cells so close together as to get an ideal combination—something which, in the trial frame, approximates to what the patient is going to wear.

There are two minor points to which I will allude: one is the dreadful waste of time caused by *steaming*. Perhaps the patient is late for the appointment, and has been bustling along, and got over-heated. Every lens placed in front of the eyes is instantly clouded with steam, so that in desperation one has to get him to go and cool down. There are two ways by which this can be overcome. We might, it is true, polish every one of the lenses with one of those cloths which contain "laxin," or preparations of a similar type. That would be substituting one waste of time for another.

The best way, I have found, is to use an electric fan. In warm weather the patient always appreciates it very much, but actually one uses it for the selfish reason that it blows off the moist air. It should be placed so that the air strikes diagonally across the face. In hot weather it is an enormous saving of time: in the cold weather it is not so much appreciated: one might set up an attack of neuralgia; but that can be got over by placing an electric radiating coil in front of the fan so that the air may be heated.

Should these means not be available, another

good method is to warm the trial case and lenses in front of the fire before commencing the day's work, and then place the tray upon a hot-water bottle. Steam, of course, will not condense on a warm surface.

The second point is *dust in the trial case*. If anyone complains that the trial case is dusty, I would ask him to dust it. He will then realise the trouble of getting into the tiny interstices, and the labour involved. A good practical tip is to blow it out with a bicycle pump. Simply squirt the air all over it until the dust and fluff is blown out.

The Trial Frame.

The trial frame is a vastly superior piece of mechanism in its earlier forms, but has, even yet, by no means reached finality in design.

Its intention, of course, is to hold the lenses in their proper positions in front of the eyes, and register the position of the axes of cylinders. Neither of these things does it do as it should. First of all, few, if any, faces are symmetrical, and yet the interpupillary distance is adjusted by means of a two-way screw. The cells ought to be *separately adjustable*, and they ought also to be *vertically adjustable*.

If there is one condition of binocular vision more essential than another in the prescription of suitable glasses, it is hyperphoria, or what is called hyperphoria. That condition in which there is a vertical deviation of the ocular axes. It is nearly always associated with an error of refraction, and therefore can often be corrected by a vertical decentration of the lens.

Can this be done by means of the trial frame? It cannot, and one wants to know why not. I would regard no trial frame as being perfect which did not allow me to make this important adjustment.

POSITION OF LENSES.

Again, I do not know of any trial frame which will allow of the lenses being placed in

the *anterior focal plane of the eye*, in all conditions of face. We often have people with bulging foreheads, and deeply set eyes. There we find that the top of the trial frame strikes the brow and holds the lenses too far away from the eyes. Again, we meet people with high and prominent bridges to their noses. Here again the lenses cannot be set back, and yet we know what an enormous difference it makes whether these lenses are in the anterior focal plane or not. We can often get 20 per cent. more visual acuity by pushing them back into the proper place.

There is no reason why the trial frame should not be so constructed as to perfectly meet the requirements of every conceivable shape of face.

Another point is the scale for *marking the axis* of the cylinder. Only those who have their reputation on the prescription can feel the immense importance of this, and yet if you take the best trial frame obtainable—costing, say, three to four pounds—you will find that the part where careless workmanship is most manifested is in this scale of degrees of axis.

It may be that it is not central with the cell. It is soldered on a little sideways. Horizontal inclinations of the axes are not very much out, but vertical ones may be out 3° or 4° . One places the cylinder in various positions, and one finds that when the cylinder is truly vertical it does not mark 90° on the trial frame. There is only one reason for that, and that is unthinking carelessness.

Then, again, that top portion of the graduated semicircle is nearly always trimmed off by a file. The eminent gentleman who makes these things gives it a final touch up before he puts it into the shop. He does not like that ugly stuck-up edge, but in taking it off to make it look nice he just takes off $2\frac{1}{2}^{\circ}$ by horizontal measurement.

I am sorry to say there is no one single trial

frame on the market, either British or foreign, which is made with anything like the intelligent care which should be devoted to it. It is not a question of price. I am perfectly willing to give £10 or £20 for an absolutely perfect trial frame, and regard it as an excellent investment. The trouble is that these things are made by workmen who do not know what they are doing. They have never seen a patient, and never used the things, and their ideas may be quite rubbishy when tested in the balance of actual experience. The whole trouble is that there is no co-operation between those who know what they want, and those who know how to supply it. I would earnestly advise manufacturers of these articles to take stock of their position, and see if this is not true. The only people who actually do know what is wanted out of a trial frame are those who are using it, and whose reputations depend on the accuracy of the prescription.

Cylindrical Lenses.

These lenses are mounted in two ways, either in *handled rims*, or in rims which are *spun on* and have no handles. Speaking generally, an optician almost invariably uses a trial frame with a rotating cell, in which the cylinder is held by means of a clip. On the other hand, the oculist always uses a drop cell, and prefers to rotate the cylinder with his finger placed against its edge, because by the latter method the speed of manipulation is decidedly greater. The handle is an advantage because there is less likelihood of soiling the marking of the lens in picking it up; on the other hand, it is a nuisance, because it stops the free rotation of the cylinder.

Therefore, as regards mounting the cylinder for rapid work, I strongly recommend the use of the spun-on rims. Unfortunately, they seem to be unobtainable in this country, although

there is no reason why there should be any difficulty.

Marking of Cylinders.

There are two points of grave criticism affecting cylinders. One is that the *axis* markings are *not invariably right*; the other is that they are *always placed on the wrong side*. The effect of a wrong marking of the axis is, of course, sufficiently obvious; but it is not so generally recognised that the error in a line parallel to the axis is actually greater than in a line which crosses the axis obliquely. I will sketch the cylinder here, and I will draw the axis markings parallel with, and a little to one side of the axis:—



FIG. 6.

Now you will see that if we read the markings at one end it is, say, 5° from 90° , that is 85° . If we turn the cylinder round, what was the upper marking now registers 5° on the other side: in other words, if we read either end of the cylinder, we double the error of such a marking.

I am sorry to say that there is a great deal of carelessness wholly inexcusable, in the way that the markings are usually done. They are often not in alignment, or, if they are, they are often not central.

Next, as regards the position of these markings. They seemed to be always placed on the curved side of the cylinder instead of the flat

side—for I assume that cylinders are practically all plano-convex or plano-concave—and the result is that if the cylinder is placed in the trial frame against the graduated margin, it has to be placed with the curved side forwards, otherwise it would not be capable of being rotated. This introduces a large error of parallax. What with the thickness of the cell, and the thickness of the mount, and the thickness of the cylinder, we may get a very considerable projection, perhaps as much as even 8 to 10 mm. Unless the eye is placed most accurately at right angles to the face and the lens, one will easily get a wrong reading of 4 to 5 or even more degrees. Now, if the markings are placed on the flat surface, that is nearest the scale, the error of parallax is correspondingly eliminated. Then why is it not done?

Retinoscopy.

For the information of those who are not engaged in ophthalmic work, I must point out that one of the best objective methods by which we can measure a defect of refraction is what is usually termed retinoscopy—although, for the matter of that, it has plenty of other names, such as skiascopy, photoscopia, etc.

If an observer reflects the light from a lamp into a patient's pupil, and looks through the sight-hole of the mirror, he will perceive the pupil illuminated with an orange glare. If now he turns the mirror on, say, its vertical axis, the diffused patch of light will appear to pass across the pupil. If the mirror is a plane one, and the patient is short-sighted, the light will pass in the opposite direction to that of the rotation of the mirror. The observer then places increased powers of concave lenses in front of the pupil until this patch of light is made to travel in the same direction as the mirror. A note is made of the reversing lens, and this practically indicates the amount of defect.

The same thing holds good with regard to hypermetropia. In this the patch moves in the same direction as the mirror, and increased powers of convex lenses are placed in front until the reversal point is reached. It is a very simple and exceedingly accurate and dependable method. It is practically our mainstay in dealing with unintelligent people, with children, and so on. Therefore any consideration given to the perfecting of the conditions under which these reflexes can best be watched would be amply repaid.

We will say this process is in operation. The patient is seated in a dark room. The darker we make the room the better will be, by contrast, the colour reflexes, and, so far, therefore,

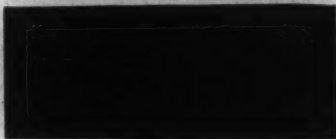


FIG. 7.

the easier will be our retinoscopy. On the other hand, the darker we make the room the less able shall we be to pick out the series of lenses from the trial case, and place them in the trial frame: so that the gain in one direction may be destroyed by the loss in the other.

We can attain working rapidity first of all with the trial frame. No one who does much retinoscopy would dream of using spring clipped cells, where every one has to be slid laterally into a known position. Here the drop cells are absolutely essential, as one can drop the lenses in, and pick them out very quickly. In selecting the lenses from the trial case we have as a rule to be content with the dim illumination of the room.

AUTOMATIC CONTROL OF LIGHT.

At one time I devised a method of having a chair on rockers, which established or broke electrical connections. Thus when bending forward to pick out a lens and place it in the trial frame the light on the patient's face was switched on, and so it could be quickly done; and on bending back to get at a distance of 1 meter or 1.3 meters at which the mirror had to be worked, that light was cut off, and the backward movement lit up the one above and behind the patient's head.

As a matter of fact, I have not relinquished that idea as useless by any means. I think it is a very good one. I only gave it up because I had something better.

A great majority of people whom I have watched at work in retinoscopy waste a good deal of time by placing their trial cases in an awkward position. It is usually on their right hand side. To pick a lens out for a patient's right eye, therefore, the operator has to pick it up with the right hand, transfer it to his left, and place it in the trial frame. The proper position for the box of lenses is almost beneath the patient's chin. The trial case table should have four rather long slender legs, without any encumbrance below the plane of the table, so that, like an invalid's bed-table, it can be placed rapidly in front of the patient's chest. In this way there is the least possible amount of movement between the rack of the trial case and the trial frame. That combined system would, I think, be most useful to many workers. I call it the "invalid table and rocking-chair" system. It certainly gives us the utmost speed of movement, and the best conditions of illumination.

A CHAIN OF LENSES.

I now come to a *mechanical device for putting up these lenses*, which I have been working at off and on for some considerable time, as I feel

that this method—if successful—has a decided future. I dare say most of you know that there have been in the past many elaborate pieces of mechanism devised for the purpose of changing glasses in front of the pupil. So far as I am aware they are all dependent on the same principle, namely, the rotation of the wheel on the periphery of which the lenses are placed. Of course, that is a very good method mechanically, but it does not work in principle, because it is difficult, on account of the movement so close to the eye, to keep the patient's gaze fixed in the proper direction. These machines always cause a good deal of nervousness, and, what is much worse, they very seldom can be placed exactly in the anterior focal plane of the eye. They are well made, and handsome pieces of apparatus, but I do not know of anyone who has ever had one who continues to use it with any enthusiasm. On the other hand, there is no doubt that one of the most prolific sources of wasted time in consultation is the changing backwards and forwards of the lenses into the trial frame, during prolonged retinoscopy—say, in the case of mixed astigmatism, with not very satisfactory reversal points.

That may arise through there being much spherical aberration of the cornea and lens, or it may be defective transparency of the media. On the other hand, there are very many cases where from one cause or another it is impossible for anyone to be certain of the exact reversal point, and one has to do it several times to make sure that it is within definite limits.

Now, all that means, as I have said, much loss of time; and every worker feels that if he could only have some method by which these lenses could be rapidly changed, it would greatly facilitate his task.

That feeling reaches its climax in modern school clinics, which have sprung into being as the result of the compulsory examination of school children. The ophthalmic work in these,

so far as the errors of refraction are concerned, is almost without exception a continuous series of retinoscopies under atropine.

The machine which I have devised depends on this principle. A canvas band passes over a



FIG. 8.

tiny roller, and is guided in its passage by a series of sprocket wheels, very much in the same way as the film of a cinematograph projector travels across its lens. Hinged to this band is

a series of full-sized trial case lenses. As the band or tape passes over the little roller these lenses rise, until, at the summit, the lens is sticking up opposite the patient's pupil. As the tape passes on, the lens is dragged down, and its place is taken by the next in series. This apparatus therefore absolutely does away with the wheel principle. The lenses are lifted by mechanical fingers, and one after another held opposite the pupil, and taken away again; and the speed with which it can be operated is far greater than can be used. I mean that the lenses can be changed in position at the rate of about 500 changes a minute; and therefore the speed of retinoscopy is entirely dependent on the operator's skill in judging the movement of the light.

A little time is wasted in setting the patient's face to this apparatus, but, after all, not longer than is needed for the proper centration of a trial frame, and the adjustment of the trial case. In practice it works out that the time spent in retinoscopies is at least half what it is under ordinary conditions; and I am quite sure that in working through a school clinic I could undertake to do three children under atropine for every one done by the ordinary method.

Retinoscopy Mirror.

Just a word with reference to the mirror used. Everyone nowadays uses a plane mirror, and if you imagine the out-patient department is being got through, or that one is engaged in examining a large number of school children, you can see that we want at once to find out whether any defect in visual acuity might be accounted for by disease visible with the ophthalmoscope. What one does ordinarily is to place the trial case aside, get out a magazine ophthalmoscope, and pursue the stereotyped method of ophthalmoscopy.

As a matter of fact, what we want in ophthalmoscopy is to sift out definitely pathological

from non-pathological cases. Direct ophthalmoscopy is rarely needed in the course of a day's work, because one examines the fundus by the indirect method, and if there is nothing remarkable, one passes on. It is only when a detail arrests the attention, that one goes on with the direct method of examining it under greater magnification.

Therefore, while retinoscopy is going on, one has the patient in the dark, with the light suitably situated, and all one has to do is to take a + 1.5 or a + 2 D.S. out of the trial frame, place it in the little clip in front of the mirror, and then one has a very good ophthalmoscope for the indirect method. A convex lens in front of a plane mirror acts as a concave mirror; and the convex lens being placed across the sight hole helps the observer to accommodate for the aerial image. It is one of the trivialities of clinical work, but it has this to be said for it, that it reduces ophthalmoscopy from an affair of minutes to an affair of seconds.

Perimeter.

The perimeter is an invaluable instrument for diagnostic purposes. Thus, in the early stages of glaucoma, when it is difficult to be sure of its presence, there is a characteristic constriction of the visual field, in which the scotoma or blind spot runs up to and includes the optic disc. Again, in optic atrophy there is a narrowing of the field, in retinitis pigmentosa, and very many other conditions. So also the field may be good at its margins, and yet be defective in its centre, constituting so-called scotoma or blind spot. I think that of all the possible affections of the visual field this is the commonest, because it is a condition caused by excessive smoking—a malady which is greatly on the increase.

The ordinary form of the perimeter you are well acquainted with. It is a quadrant graduated on its margin in terms of angular deviation

from the centre of the sphere, and which can be rotated about the line of fixation of the eye, so that it becomes a solid figure—that is to say, a hemisphere.

Now we will assume that we have this very common condition of *central scotoma*, and we are provided by the maker with a perimeter for its measurement. The patient has one eye blindfolded, the other is placed in the axis of

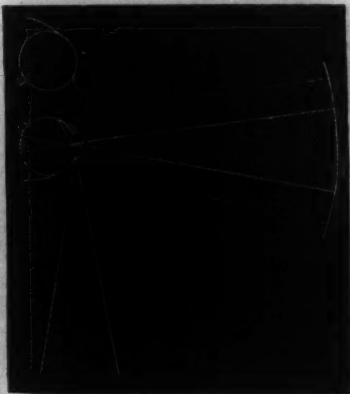


FIG. 9.

the instrument. He is told to keep his eye on the fixation spot, and then the peripheral object is moved outwards until it ceases to be seen. Now, that is very pretty, and seems easy enough; but I can only assume that no instrument maker has ever asked himself, "How is a man with a central scotoma to fix the fixation object on the axis of the instrument?" That is just where his blind area is. So in practice what happens is this. Projecting outwards into

space from the patient's eye is a cone of blindness, and occupying that cone is the fixation object you are asking him to see. He has obviously to rotate his eye in some direction so that the sentient margin of the cone can become aware of the presence of the object. Now it all depends which side of the cone he uses. If it is the side that you are measuring, you will find vision right up to the fixation spot, and if it is on the opposite of the field you are measuring, you will find the scotoma twice the diameter it really is. Now, there is a beautiful state of affairs! The plain fact is that the perimeter is absolutely and totally useless in its present construction for such an estimation.

A lot of time and thought has been given to furnishing it with a sliding test object, and *gearing down a needle* which will prick the test chart; but, unfortunately, owing to ineffective workmanship and design, these geared down mechanisms are worse than useless because, owing to backlash and looseness of working parts, and insufficient adjustments, they nearly always register with a great margin of error.

So much so that, personally, I would not trust, under any conditions, a mechanically pricked out chart of a field of vision.

Now, there is another point, if you look at a printed chart of a field you will see that the visual area extends beyond 90° : that while looking ahead we can see sideways to a considerable extent behind us—in short, on the temporal side the margin is 110° or even 115° , in some eyes 120° . This is a most sensitive portion, it is the first which shows constriction of the visual field, and yet it *cannot be measured* simply because the instruments are so badly made.

When a workman starts to make a perimeter he takes an arm, which is an arc of 90° , and then he mounts upon it this sliding mechanism which requires support; and then, with one thing and another the actual movement of that test object covers an aperture of only about 75° .

You can look at any perimenter you like, which is mechanically propelled, and you will find my words are true. The actual effective aperture of that instrument varies from central, and very badly central, up to about 80° , certainly no more. Now, why in the name of all that is sensible, should that arm not be brought round so as to make the whole field measurable, and that in its most valuable portion? There is no reason at all, except ignorance.

Fusion Testing and Training.

A field of ophthalmic work which is as yet almost untrodden is that of the study of binocular vision. The time is gone past when it was sufficient for an ophthalmic surgeon to cover one eye, and test the exposed one; then cover it up, and test the other. It was generally held that when that was completed, one could sit down with confidence and write the prescription. But nowadays things are getting different. We recognise that an enormous amount of eye strain is due to anomalies of what is called the muscular balance.

Before I touch upon instruments, may I just point out that, although it is perhaps convenient to speak of muscles being at fault, we have to remember that the muscle is only a servant of a nerve centre, and that actually the muscles themselves are perfect.

MUSCULAR CO-ORDINATION.

As I have said elsewhere, if you teach a person to swim or ride a bicycle, you have not strengthened his muscles—it has probably not made the slightest difference—but you have co-ordinated them. What we aim at is the establishment of a normal co-ordinated reflex. That is what skill and practice means.

Let me take, as an analogy, a child learning the violin. Its attention is fixed on fingering the strings by the left hand, and also it has to think of the right way of holding the bow: beyond

that to consider the note it is playing, its meaning, duration, quality of tone, trueness of note, and so forth. You can imagine that a tired and snappy teacher would find fault with it for holding its bow wrongly, while the pupil is honestly considering whether the left hand is properly placed to take the next passage—in other words, on the movements; and mental efforts are directed from the reasoning portions of the brain entirely. But as that child acquires skill, the handling of the bow, and the manipulations of the left hand become more and more unconscious, they are no longer directed to the higher centres of the brain, but are dealt with by the lower centres; and the higher centres of the brain are thus released for fresh mental efforts, leaving the fundamental ones to be automatic. Hence in the highest expression of a trained player the only point on which his mind need be engaged is the interpretation of the spirit of the composer.

A second point has to be borne in mind in connection with deviations, and that is that in all cases we aim at the re-introduction of an instinctive reflex which has been handed down for countless generations. There is therefore an enormous sub-conscious tendency to be able to do it. Now, that is a very different thing from endeavouring to establish an abnormal state of muscular balance. At present we are passing through a stage when muscular deviations and muscular trainings are comprehended but by a few. It might be fair to say that systematic treatment is scarcely done at all. It behoves everyone, therefore, who is interested in the manufacture of instruments, to try and comprehend what is wanted and what line of development is indicated.

Now, first of all, as regards testing. The little bundle of rods which Mr. Berry recommended to Dr. Maddox, has been found to be of the greatest value in telling us whether muscular imbalance is there or not. But if this imbalance is asso-

ciated, as it nearly always is, with an error of refraction, so much the more is it imperatively necessary that the trial frame should be capable of the most perfect centration, as otherwise it is incidentally obvious that we can manufacture no end of imbalance by careless setting.

I shew you here what I believe is the *only objective method of demonstrating imbalance*.

Demonstration.

You know that in the ordinary way the looker-on can see nothing of the red streak. Here I have two mono-chromatic lamps of feeble luminosity, and therefore easily suppressible, yet luminous enough to be readily visible: these lamps move on a tramway under the control of the operator at the patient's end of the room. Muscular imbalance assumes a new depth and gravity of meaning when one sees a person setting them two feet apart, and saying they are directly over one another.

As arising out of that, we come to the question of treatment or training. In that, of course, we are dependent on the stereoscope, either in its original form of Wheatstone—as exemplified in the so-called amblyoscope—or in one of its other modifications, principally Brewster's.

I show you here an amblyoscope, which, although a crude instrument, has done good service in drawing the attention of the ophthalmological world, as has been done by Mr. Worth, to the splendid pioneer work done by Javal. It is to Javal that we owe practically the whole of the foundation of the knowledge of treatment.

The mechanical disabilities of the instrument are that there is no mechanism for altering the *interpupillary distance*, and as the lenses placed in the eye are of great power that is a most important point.

Another point I show you here: Supposing the tubes are set for correct interpupillary distance, since they are turned inwards to deal with squint cases, it is *impossible* for the child to get the conditions of binocular vision. I have long

felt that we have not been fully conscious of the significance of fine movements. In other words, we speak of a 4° or a 5° prism as a strong one, and yet we make a movement of these tubes which would represent the angular displacement of a vastly more powerful prism. A little movement in an amblyoscope corresponds to a very great deviation in a prism.

Another point, I felt, was that the pictures used in these instruments were *far too coarse and primitive* to appeal to our stereoscopic sense. We want as close an approximation to the actual thing as possible, and that, of course, can be done by stereoscopically photographed lantern slides mounted in the ends of the instrument.

I show you here a new model of my training apparatus:—



FIG. 10.

Showing how tubes adjusted for parallel eyes are centrally fixed, but when the joint is closed, the axes no longer coincide.

You see that the patient's face is firmly fixed: there is an *adjustment for interpupillary breadth*; the tubes are so hinged below the patient's chin that *their vertical axes correspond to the centres of rotation of the eyes*. In all excursions they are concentric with the movements of the visual axes. We get as perfect a representation as it is possible to get, and the movements of the instrument are so exceed-

ingly fine and registered that one can use the movement of the tube in interchangeable terms of the axis of a prism. We can deal, as you see, with defects of convergence and divergence, of supravergence, and of cyclovergence. The main point that I rely on is this: we establish in any condition of balance a true stereoscopic conception, and having attained that we find that the mind resists the splitting up of this picture so strongly, that we can establish opposite tendencies to an enormously greater extent than could be done by other means, before the binocular sense gives way and the picture is broken up.

Ophthalmometer.

In view of the fact that my friend, Mr. Sutcliffe, one of our past Presidents, will address the Society later on concerning corneal measurements, I do not think that it is necessary for me to touch upon the ophthalmometer



FIG. 11.

In the Synoptophore, the centre of rotation of the tubes is under that of the eyes. The simultaneous picture can therefore be used to draw the axes into parallelism.

from the point of view of design. He has done so much, and his own designs are marked with so much originality that, although I have a few ideas as to what might be done to improve any instrument, from the clinical standpoint, I will reserve those suggestions for his evening.

Discussion on Dr. Ettles' Presidential Address.

MR. CHALMERS, in proposing a vote of thanks to the President for his very able address, said one of the chief objects of such a Society as the Optical Society was to bring makers and users of optical instruments into closer touch with each other. He could not help saying, however, that Dr. Ettles had himself to blame in some ways for the shortcomings of which he had complained. Many years ago he might have said some of the things he had now said to the Optical Society, and those things might have borne fruit; but he had neglected to do so. That was true of a great many other people; they grumbled at the instruments they were using, but they never thought of telling the men who were making them that they were not trying to produce the things which the users wanted; the users were content to keep on waiting until they could themselves design something better. Now, most instrument makers were quite capable of improving the actual design if only they were told that what they were trying to do was not what was wanted, and that was one of the things which ophthalmic surgeons had hitherto rather failed to do; they had not criticised existing instruments in the way Dr. Ettles had so effectively done that evening. Dr. Ettles had referred to the method of illuminating the test chart for which he (the speaker) was jointly responsible with Mr. Mackinney, and had said that there was a less perfect illumination at the foot of the chart, where, because the letters were smaller, a better light was required. Now, it was just as easy to place the mirror below the chart as above, but the right solution undoubtedly was to have the mirror, as originally designed, to extend the full length of the chart. Here the question of price came in, and it was not thought desirable to obtain any more perfect evenness of illumination by using two lengths of mirror. But with the longer mirror the illumination was almost as perfect as one

would desire to have it. There was one suggestion he would like to make to Dr. Ettles, and that was that he was trying to get too much out of one trial frame. He thought that was the solution of the difficulty—that one trial frame could not be made to do all that was required in all cases. If Dr. Ettles was prepared to spend, as he said, ten or fifteen guineas on a trial frame, an expenditure of less than that on a selection of trial frames might quite well meet most of his requirements. One or two of the cases mentioned by Dr. Ettles were obviously cases for specially-designed trial frames. The stereoscope slides used by Dr. Ettles in connection with the amblyoscope were very valuable, and had been used by nearly all those who had seriously employed that instrument. In certain special cases particular types of objects had been found extremely useful; for instance, balloons had been found particularly good objects, and he believed the general experience was that ordinary photographs were not nearly so good as black and white diagrammatic stereoscopic photographs.

MR. W. A. DIXEY, in seconding the vote of thanks, said that it gave him considerable gratification to hear the President's invitation to his audience to comment upon the different subjects dealt with in his address. To most of those in the room the subjects treated of were very familiar, and one of the advantages of such familiarity was that one could afford to indulge more freely in criticism. With regard to test types, he did not know whether it had occurred to Dr. Ettles that there was a very simple and practicable method (it had been adopted, he knew, by many people) of grading the test types more finely even though only the ordinary types were employed—namely, by regulating the distances at which they were seen. There were two factors which went to the estimation of acuity of sight: one was the size of the letters, the other the distance at which the letters were seen. Obvi-

ously the gradations might be increased by multiplying the letters, but that could also be done equally well by dividing the distances. Dr. Ettles had commented on the shape of certain letters for which he (Mr. Dixey) was, he was afraid, responsible. He admitted some of them were a little eccentric in form, but he could assure them those letters had been devised with a full appreciation of the rule Dr. Snelling had originally laid down. Not only did each letter subtend a definite angle, but each limb of each letter. And what was more important still, the space between each limb of each letter subtended a definite angle. It was too long a subject to go into fully, but the letters had been, he was going to say intelligently designed—(laughter)—but at any rate they had been deliberately designed. He would give an illustration which they would all appreciate. Taking the letter O or the letter C, it was easy to draw that letter of such a size as that the whole letter subtended a given angle, but an O could not be drawn (unless it was a very thin and skinny O) in such a way as that not only the limbs but the interstices between the limbs subtended the angle laid down by Snelling. And it was to fulfil that condition that he had designed some of the letters which Dr. Ettles had criticised. With regard to the test case, the paucity of the number of lenses in the test case was obviously a matter for the oculist who gave the order. All through his business career he had made test cases, and quite recently he had made a test case with single diopters right up to 20 D.; indeed, he was not sure that he had not gone beyond 20 D. So that he did not think that in this respect there was any real subject for complaint, or if there were, the remedy lay in the hands of the doctors themselves. Many manufacturers would be delighted to take Dr. Ettles order for a trial case with any number of lenses he liked to specify. (Laughter.) With regard to thin lenses, it had been quite a common prac-

tice for years and years to supply the deep lenses in the thin form, and to champfer the concave lenses; and with regard to the convex lenses, quite a practical way was to mount the thin lens in a transparent, light-coloured celluloid visual. In his opinion it was not a good method to cement the lens on a plain glass. With regard to the question of dust getting into the trial case, he could not believe that Dr. Ettles had not seen the trial cases in which the whole thing was made to lift bodily out. Such a trial case was quite easy to dust, though, of course, in order to dust the lenses themselves each individual one would have to be taken out.

THE PRESIDENT: I had one, but could not keep it clean; the dust was left all along the edges.

THE BICYCLE PUMP.

MR. W. A. DIXEY: Then in such a case, of course, the bicycle pump would be a good method. (Laughter.) Proceeding, Mr. Dixey agreed with Mr. Chalmers' remark, that in order to fill all the requirements it was possible to wish for in a trial frame, more than one frame was desirable. One great point in an ordinary working trial frame was simplicity. For that reason, if a man were going to rely on a single trial frame, he would not recommend him a very elaborate one. Such a one, for instance, was Dr. Bardaley's frame, which had a special apparatus for adjusting each eye separately, horizontally, of course. The vertical adjustment was another matter. He confessed he had never seen a trial frame in which it was possible to get vertical adjustment, and for various reasons he did not attach, perhaps, quite so much importance to that point as Dr. Ettles appeared to do. He would give them a hint as to why that was when he came to speak about heterophoria. With regard to the cells, they must have noticed his (Mr. Dixey's) reluctance to father the diagram drawn by Dr. Ettles.

At the same time, though somewhat exaggerated, it was more or less what one saw in certain trial frames. Some time ago he had got over the difficulty by making four-fold grooves, in which the thinner lenses could be put in close juxtaposition. In the case of the thicker lenses they simply omitted one of the grooves, putting one lens in No. 1 groove, and the next in No. 3, or even in No. 4. Now it was just as easy to make a frame like that, as long as the work was good. Of course the necessary conditions were that the grooves should be very accurately shaped, and also that the trial lenses should be truly circular. Dr. Ettles had said they were dependent upon Germany for spun trial lenses. Now that was absolutely incorrect. (Hear, hear.) Oddly enough, only yesterday afternoon he had gone up to his instrument workroom and found one of his workmen making spun lenses; the lathe was turning and the lenses were dropping out, any number of them—(laughter)—and if anyone liked to order any number of gross of spun lenses from a certain workshop in Bond Street he was quite sure they could be supplied at once. And not only there, but at lots of other places, there was no difficulty about it. He was very glad Dr. Ettles had mentioned the question of centering cylinders. This was a very important point. In centering a cylinder in a test lens, it should be recognised that the essential thing was that the axis mark should be in the real geometrical axis of the rim, and not, as more often it was, exactly on the apex of the cylindrical curve. It ought to be both, but perfection in this matter was impossible; there was sure to be an error of $1/100$ th mm. or so. His point was that the error should be in the centering of the lens, and not in the fixing of the glass into the rim, because if there were an error in the adjustment of the axis mark to the central diameter of the rim, there would be a considerable error in reading off the axis on the scale; whereas if

the axis mark was in the geometrical axis of the rim, the error would be only a very trifling one of decentration. He feared he was trespassing unduly on the patience of the meeting, but he did want to say one word on the subject of heterophoria and stereoscopic vision. He mistrusted all these tests for heterophoria, on grounds to which Dr. Ettles had himself called attention more clearly than anyone else he knew.

He would not attempt to go deeply into the matter, but would content himself with calling attention to what Dr. Ettles had already published on the subject. With regard to Dr. Ettles' test for heterophoria, which he had shown them that night, he, the speaker, mistrusted the notion of putting a person into a dark room with a couple of weird-looking lights, at which that person was to look at an undefined distance, and then testing that person's heterophoria. The person, unless he was very intelligent, did not know what the distance was, his accommodation would wander about, his convergence would wander about, and no sort of clear result would be obtained. It seemed to him the proper way to test heterophoria was by testing the range of convergence under a given degree of accommodation; that, in his opinion, was the only right way to do it, and the best means of doing that was by a properly arranged stereoscope. The stereoscopic arrangement shown by Dr. Ettles was excellent, but he thought there were others which were better because simpler. He could not mention them without giving names, and he did not want to do that, but he was speaking of a stereoscope where the accommodation was fixable, and where the amount of convergence could be varied by the use of rotating prisms. That was the best device for measuring the range of convergence, and the best method of treating cases of squint and so on. In doing that it was quite right, as Dr. Ettles had pointed out, to use the sense of fusion, and if a stronger incentive than the

sense of fusion was needed, they had that in the stereoscope. The subject was a rather involved one, and it would be a mistake to try and deal with it sketchily. It was a subject concerning which there was a good deal to say that, as far as he knew, never had been said, but he would therefore, on the present occasion, bring his remarks to a conclusion without entering any further into so enticing a subject.

DR. R. S. CLAY cordially supported the vote of thanks. Dr. Ettles had modestly said it was "not much of a Presidential Address," but it seemed to him one of the most practical Presidential Addresses to the Optical Society that he had heard. (Hear, hear.) He was sure that to anyone connected with the manufacture of ophthalmic instruments the suggestions thrown out by Dr. Ettles would prove most valuable. There were two points he would like to refer to. One was, whether there was any need to use such large lenses for retinoscopy and in sight-testing generally. It was usually said that if small lenses were used, people imagined that they were looking through a tube, and that this caused them to accommodate, and brought about wrong results. He would like to ask Dr. Ettles whether the objection had any real physiological basis. If the lenses could be made smaller one could obviously reduce the size of the trial cases: the frame also could be made smaller and lighter. One could also more easily, in the case of a deep-seated eye, get the lenses into the nodal plane of the eye. No doubt, if the lenses had not handles, they would be awkward to use. The other point to which he would like to refer was the design of the instruments for heterophoria. Mr. Dixey had suggested that other types of instruments (with rotating prisms or mirrors) might be as good as the one Dr. Ettles exhibited. Now, it seemed to him that the same fallacy underlies the use of these instruments as that which Dr. Ettles had referred to in another connection, namely, that whilst they would be

all right for a person without diplopia, a person who squinted would look through the rotating prisms or mirrors obliquely. Nor could he see any way of avoiding this, except by the adoption of the principle of rotation round a vertical axis through the centre of rotation of the eye. Dr. Ettles' instrument was the first he had seen based on this principle, and therefore it was a real advance upon anything he, at any rate, had heard of in that connection.

Reply.

THE PRESIDENT, in acknowledging the vote of thanks, said, as regarded trial frames, it was true, as had been pointed out by both Mr. Chalmers and Mr. Dixey, that two trial frames were needed: one with the coarse adjustment and drop cells, which enabled them to do their retinoscopy and "roughing out," and one with a fine adjustment, by means of which they got perfect reading of the axis, and so on. His trouble was that he could not get the fine one fine enough to suit all classes of faces. In dealing with a case of hyperphoria it ought to be possible by decentration lenses to establish orthophoria. As things were, he had to use Silvanus Thompson's tables. If it was an ordinary case, without any astigmatism, he simply tilted the trial frame sideways, one spherical lens went up and the other came down, and he got a kind of correction that way. But, surely, that was not good enough.

MR. W. A. DIXEY: Why do you not use a graduated double rotated prism?

THE PRESIDENT: It cannot be got in the trial frame in combination with lenses already there. It would be much easier to be able to move the cells up and down. Continuing, Dr. Ettles said Mr. Chalmers had remarked that there ought to be two holophane reflectors, one above and one below; but the mirror did quite well. Then Mr. Dixey had told them they could do away with a chart such as he, the speaker, had shown them, and move the test chart backwards and for-

wards. But this would necessitate a very complicated mechanism, as it would have to be moved backwards and forwards for each intervening step

MR. DIXEY: It is only a possibility.

THE PRESIDENT dwelt on the question of intrinsic legibility of letters of test charts. It was not enough to design a letter which presented constant proportions of black and white—it must be capable of easy recognition as a proper letter. If a person read only the O's and L's in 6/6 he was not registered as having 6/6 vision; the actual value would perhaps be only 6/9. But if he read the K, H and M, they knew he really had read it, and then the point of intrinsic legibility was most valuable as giving a sort of assessment of what a patient really could see. With regard to the series of lenses he had suggested, he knew an ophthalmic surgeon could go into an establishment and say, "I would like a trial case to this specification," but what he wished to point out was that the vast majority of trial cases were sold out of stock to beginners, who did not exactly know what they wanted, and what he said was that that stock case was not the best design.

The point raised by Mr. Dixey about the four grooves certainly got over his difficulty; it was quite a good point, and he did not know such a plan had been adopted. But when Mr. Dixey talked about "mistrusting" his test for heterophoria, he had a very simple answer, namely, that people whose eyes were all right never made a mistake, and that people whose eyes were wrong did make mistakes. Then with regard to prisms, he feared Mr. Dixey was thinking about one thing while he, the speaker, was thinking of another.

MR. DIXEY: I was thinking of that principle in connection with the stereoscope.

THE PRESIDENT said if they took the case of a child with a squint, one eye directed straight, the other turned outwards, the brain automati-

cally switched off one image on to the other; the cerebration was gone—the whole mental conception was gone.

MR. DIXEY: We are thinking about different things. I was not thinking of squints at all like that; we were talking about heterophoria.

THE PRESIDENT: That apparatus is for training squints.

MR. DIXEY: I was not talking about that instrument at all, but about heterophoria tests.

THE PRESIDENT said for heterophoria it was no use unless they had such long levers, and the dial so finely graduated that the prisms and movements were interchangeable terms. After all, the prism only deflected something. They could either deflect the image of that something or deflect the something itself; it did not matter which they did. Therefore prism powers and movements ought to be scientifically interchangeable. The beauty of the instrument, so far as he could claim any beauty for it at all, was that he had got the movements so exact and fine, that when speaking of the movements they could know what it meant in terms of prisms.

It was usual to think of prisms as being one kind of thing, and of movements as being another kind of thing, whereas really they were the same thing, or ought to be.

ON THE DESIGN, DETAILS OF
CONSTRUCTION, AND USE
OF TRENCH PERISCOPES.

DISCUSSION INTRODUCED BY MR. W. A. DIXEY,
4th February, 1916.

MR. W. A. DIXEY: Mr. President and Gentlemen, I would like you to understand first of all that this is not a paper I am going to read. I have simply been asked to introduce a discussion. If I had undertaken to read a paper on periscopes, I should have thought it my duty to acquaint myself with all the different types of periscopes that are being used and manufactured now; and that I never contemplated doing. It is rather by accident that I am in the position of introducing this discussion.

The trench periscope is an instrument which a great number of us opticians are engaged in manufacturing; and for the last few weeks or months we have been devoting a good deal of attention to it. So that it seems that a discussion on the instrument may not be inapposite at a meeting of our Society.

The trench periscope, as it is ordinarily conceived and made, is a very simple instrument. The object of it, as you know, is to give the soldier a view of his front whilst his head and person are sheltered. This object is ordinarily effected by means of a couple of mirrors, so adjusted as to reflect the view in front. Nothing could be simpler, but, as is the case with many other very simple instruments, there is considerable scope for variety in manufacturing this utensil, and already, although it is not a thing

that has been made for a very long time, it has assumed very many different guises.

For those of us who are making the periscope it is a matter of some interest to try and establish what really is the best practical form for this instrument, because there are really many conditions in connection with it which require consideration. It is not at all a remarkable thing that it has not assumed a final form yet, if you think of the length of time that it has taken to produce the perfect bicycle—even assuming that to be yet produced—or the perfect motor car. Or, to take a simpler instrument, those who are old enough to remember golfing twenty or thirty years ago, if they compare the clubs you put into your bag then with those used to-day, will find that even so simple a thing as a stick to hit a ball with, or a golf driver, has assumed very different forms in the course of its development. So that it is perfectly natural that this simple instrument, the periscope, should also assume different forms, and that the final form of it should not be arrived at without some lapse of time.

Now, in considering what form to put your periscope into, there are two or three factors in the problem, and I have set down here for our consideration four principal ones. There are, of course, some qualities which do not invite discussion; everyone will agree upon such points as excellence of manufacture, the best possible definition, and so on; but there are some things that are rightly variable, and to a certain extent mutually antagonistic, and want balancing—and those are the points which I think we ought to discuss to-night. Now, the four points about the periscope I want to call your attention to this evening are:—(1) Portability. Obviously the periscope ought to be portable. (2) The degree of shelter which it is desirable for it to give. That very much resolves itself into the question of its length. (3) The size of field that is necessary or desirable. (4) What I call its

obscurity, because I cannot find a better word, but what I mean is the nature of the target that it offers to the enemy—how easy it is to shoot at it from the opposite trenches.

Well, first of all, with regard to portability. I think the simplest way for me to illustrate that is to show you, not as a necessarily final thing, the sort of solution which we have reached ourselves. (Mr. Dixey here showed a periscope in the form of a garden syringe, consisting of 21 inches of khaki-coloured brass tube, with a wooden handle. Apertures are cut in the tube, the upper one two inches, the lower one one inch in diameter, and the instrument is fitted with plane silvered glass mirrors.) We have been making these things for two or three months, and we have arrived at a sort of solution which, as I say, I do not present to you as final, but only to illustrate some of the points which we are considering. Lightness is one point. Here you see you have a periscope made of a bit of tube—this happens to be a brass one, because it is the only one I could get hold of, for obvious reasons. But it could be made equally well, and we are making it, in aluminium; and aluminium is perhaps the best for portability. The form of the utensil was also, I think, arrived at mainly with a view to portability. It is not only easily carried, but it is portable in this sense—that when it is being used it is very easily turned about. It is a convenient, carryable size as compared with some others which have no pretensions to portability. I have seen pictures of quite large-sized periscopes; almost large enough for two heads to look into together, so big that when they are carried they have to be carried on a man's back. Some of the other square-shaped periscopes that one has seen are also not so portable as the one that I show you here. There is one that is familiar to you all, that has been made in very large quantities, that is portable, but which has other qualities which we shall consider, and which, I think,

make it less desirable than some of the others. The one I refer to is the one which consists of two slips of wood, into which are fitted the mirrors which may be attached to the top and bottom of the frame.

Height Required.

Now, let us switch off this first quality, and get on to the second, which is the degree of shelter desirable to be given. That is rather an interesting point. I suppose everybody in this room has read a letter which has been rather widely published in the newspapers, written by Captain de Crespigny. I think that letter began the popular agitation about periscopes, or went largely in that direction. In that letter Captain de Crespigny, who is a practical man who has seen a good deal of trench work, laid it down that a periscope should be at least three feet—not less than three feet—long. Captain de Crespigny's experience of the trenches has been of the older form of trench, where the rubble and the loose earth have been thrown up in front of the trench. His point was that you wanted your periscope to get above this heap of loose earth, and that the earth itself was no shelter; the hostile bullet went through it like cheese or butter, and for shelter you had to get your head right under the trench. Therefore you had to have at least three feet of periscope to clear this loose earth. In talking to Captain de Crespigny I suggested that it ought to be possible to make some sort of shelter, of railway sleepers, or blocks of wood or sandbags, or something built into the rubble, behind which you might use the periscope, because a thing three feet long is very unwieldy, and it is very desirable to reduce the length. Well, I may say, he sanctioned it in the case of the periscopes I made for him, and the length was reduced to 2 ft. Since then I have been told by two or three men who have come from the front that trenches are being made in what, to me, as an

ordinary common-sense civilian; seems the obvious way; the earth is thrown behind instead of in front, and the front of the trench is beaten down hard, and there is shelter to within about three or four inches of the top of the trench. You see that affects very materially the question of the length of your periscope. Practically the result is this: that we are now making a standard periscope, with the sanction of the War Office, of 18 inches between the mirrors, or 21 inches length of tube. Those are the dimensions of the instrument that I have here to-night, and it is pretty obvious that there is ample provision for shelter if you have a hard, rigid, more or less straight-edged barricade in front of you. So that, as far as we are concerned, we have solved the question of length, or the question has resolved itself into a length of 18 inches or 21 inches. But if I were asked to take a standard I should take 18 inches, and I should be prepared to contend that in present circumstances, as I understand them, that is amply long enough, as compared with three feet length, or anything approaching three feet, which has been advocated.

Size of Field.

Well, now comes another important point—the size of the field. This instrument justifies its name of periscope by the fact that when you are looking through it it is very easily turned round. Its quality of elevating the line of sight would rather justify the name of hyperscope. However, it has got to be commonly recognised as periscope, or trench periscope; and there is no real objection to that name. By the way, I have seen somewhere the word “suprascopes,” which is one of those hybrid words which ought on all occasions to be barred and forbidden. (Laughter.) Well, now, I am talking about the size of the field. When you first see this instrument, and perhaps mentally compare it with one with larger

mirrors, you are inclined to say, "Oh, the field is very small!" Well, as a matter of fact, this gives a field of six degrees, and to those of you who are familiar with the size of the average binocular, six degrees will not seem an exceptionally small field. If you want a bigger field there are obviously only two ways of getting it with this type of instrument. One is to shorten the length between the two mirrors, which is undesirable, and the second is to make the diameter bigger, which is also undesirable on account of the weight, and general handiness of the instrument. Six degrees is the size of field which has been given to me by the War Office, and which in my War Office contract is the size of field to which we have adhered. The tangent of 6° is .105, i.e., it covers 10 yards at a distance of 100. Or, the better to convey the idea, you see the height of a man at 20 yards, and that really, as a matter of practice, seems to me a sufficient field, in view of the fact that you can extend it laterally by turning it round and so cover the whole length of the trench. If you have the periscope made so that it has to be fixed in the earth, then you can only just cover that part of the front which the size of the mirror allows.

The fourth point which I suggest we consider in this discussion is the obscurity or the degree of target that your periscope presents to the hostile trench, because, obviously, if you have a mirror about the size of a brick projecting above a trench, and there is a man at 100 yards or a couple of hundred yards—and some of the trenches are distant only 50 yards—that must be exposed to his view. If he is to be seen, *ex hypothesi*, it is exposed to his view, and it affords a very easy target; and I imagine it would not be in the least difficult for a quite indifferent shot to put a bullet into it at 100 yards in quite a large percentage of shots.

Degree of Obscurity.

Visibility is another point. With an open periscope that is not in a tube or box, or anything of that sort, the mirror is obviously illuminated, and a hostile observer in the opposite trenches, in looking into the top mirror, will see the face of the man looking into the lower mirror. I saw a letter in the paper—I dare say, most of you saw it—about a German periscope that was used in that way, in a mirror in which the opposite English could see the face of the German officer who was looking through the periscope, and this periscope was so large that he could see the whole of his face and his mouth moving, making a joke to his friends in the trench, and he could even see part of his collar and shoulder straps, so as to be able to identify the regiment to which the German belonged. That I read in an evening paper, so I suppose it must be true. (Laughter.) However, you see the point; you have got to be careful that your bottom mirror is not too well illuminated, because whatever is opposite your bottom mirror is visible to the object you are looking at; and, if that object is a man's eye in the opposite trench he sees in this top mirror what is in front of the bottom mirror. Now, if you put your mirror into a tube, it is fairly dark inside, and practically invisible. Then, with regard to the invisibility of the framework of the instrument, there seems to me to be a point in favour of this tubular arrangement, which I am showing you here to-night. It seems to me that if one is nervous about the invisibility of one's instrument, one can quite easily remedy that. I am told that a handful of damp clay is not altogether an unattainable thing in the country where they are fighting. (Laughter.) If the top be smeared with damp clay, and a few bits of dry grass or dead leaves stuck on, it would make the thing practically invisible in the hostile trench.

Stand Periscopes.

Well, now, I want, before I make way for more interesting talkers, to mention one or two things. I have been living with these instruments for the last two months, and I have had all sorts of things suggested to me, and have read all sorts of weird things with regard to people's ideas, some of them quite interesting and some only amusing. One very common demand is to have a stand for these instruments. I do not believe in it myself, because, as I have explained already, it seems advantageous to be able to turn the periscope about, but obviously you can make it quite easily on a tripod stand. If you are going to make a bigger thing altogether, then the tripod stand comes more into the picture—if you are making it three or four feet long, it would have to be wider—of three- or four-inch tube, then you would have a very much heavier instrument, and obviously it would be a rather difficult thing to hold up. Well, that might be put on a stand, and if that is done it ought to be on something in the nature of a ball-and-socket joint, so that you could turn it about with your hand, and it ought to be capable of being tilted a little so as to cover the whole field. If a periscope really became an established instrument, and it were desirable to have it in a fixed position, I think something of that sort might be as well, although it would not have any great advantage over the smaller one. I may say that another one which I have made, which I do not recommend, is a bullet-proof one. It was terribly heavy work, made of about $\frac{1}{2}$ inch steel tube, two inches in diameter inside, I think; and the weight of it was something terrific.

MR. CHALMERS: Were the mirrors of glass?

MR. W. A. DIXEY: Yes. If you make it bullet proof, you only want about six inches at the top bullet proof; you do not want it bullet

proof all over. Well, here is another problem I have had, and I have refused to do it because it is not practicable—at least, not practicable for me. It wants a different kind of workshop to mine. But it is not a bad idea, and is rather amusing. That is, to have a periscope which may be attached to the rifle. You see the obvious advantage of it. The rifleman is completely sheltered while firing, but it has got to be made and fitted with such precision that it is really a rifle sight as well. You have got to be able to adjust your periscope to use it as a rifle sight, and then, of course, your rifleman can shoot with just sufficient loophole for his barrel to go through. It is theoretically possible, but I would not undertake it, and there would be very great difficulty in the rifleman using it, because he has got to have his sighting arrangement absolutely vertical, and if he errs the least from the vertical his bullets will err, and at such a great height as this the smallest error in the judgment of the vertical position of his sight would lead to serious error. I do not think that is feasible, and it is a thing I should not try to carry out myself. Then there is another thing.

The Backward View.

A man came to me and made a complaint. He had been driving all over the country in an armoured car, and was always getting his chauffeur shot—(laughter)—which was very annoying. The chauffeur had not only to look ahead, but to look behind, and in order to do that he had to stick his head out of the armoured car, and if a shower of shrapnel was falling, he was very often hit. And the man who came to me wanted something in the nature of a periscope which would enable the chauffeur to see behind and in front as well. It is a very pretty problem, and one's first thought was to arrange a periscope vertically with a V-shaped double mirror at top. One

would then get a horizontally divided field; the upper half would show the front view, and the lower half the rear view. But the rear view would show everything upside down. That would make driving very awkward; and there is another way of solving the problem. Let the periscope project laterally from the car. The end mirrors should be set one above the other at right angles to each other, the upper one reflecting the front and the lower one the rear view.

The chauffeur, when he looks through his peep hole, would have the same sort of divided field as before, only the rear view, instead of being upside down, would be right side up. Well, that is a suggestion for an instrument that I never made, and I do not know that anybody else ever did.

There is one other thing that occurred to me in thinking about what I should say this evening. I have not yet done it, because I have not had time. There is such a rush for the ordinary form of periscope that one has not been able to go into all these improvements that one might think of. But in an instrument of this sort it might be useful in the field if the top mirror were graticuled—that is to say, if a scale were put on the top mirror. In the case of this periscope the angle covered is about six degrees. Or, rather, let us put it this way, which makes it simpler. The length between the mirrors is 18 inches, and if you have a couple of marks on the top mirror, separated by some divisible of 18 inches, say 1.8 inches, which would just about fill it, you have a ratio of one-tenth. Now, if you divide this space into tenths, each one of those divisions is in the ratio of one to 100. And that might be useful in this way. If you know the height of a distant object to be a yard, and it just covers one of these $1/100$ th divisions, you multiply the yard by 100, and you know the object is 100 yards away. And it occurs to me that such an

arrangement might possibly be useful for the rough estimation of distance.

Magnification.

Well, now, gentlemen, I have kept you rather longer than I meant to, but there is still one more thing to talk about. In this instrument there are simple plane mirrors. Of course, the ideal instrument would be made not with silvered glass mirrors but with rectangular prisms. Well, for the reasons you are all acquainted with, we cannot get in any quantity rectangular prisms big enough even for a small instrument like this, and moreover it would make the instrument very much more expensive than it otherwise is, and it is quite practical with decently surfaced silvered glass. It has no magnifying quality, and that to a certain extent is a drawback. Obviously the instrument would be more valuable if one could give it magnifying power. It is possible to fit a set of Galilean lenses in front of the lower mirror, and so give it a magnifying power of $2\frac{1}{2}$ or 3 diameters. But in this respect a silvered glass mirror has one serious disadvantage. If the mirrors are carefully set in the instrument the disadvantage is not very obvious, and not very much noticed, but it is there. But for some reason or other, when you put a system of lenses on it the drawback is very much more in evidence, and it interferes very considerably with the effectiveness of the instrument. Now, I want to tell you what the drawback is. In a silvered glass mirror there are two reflecting surfaces. A portion of an incident light ray is reflected from the first surface of the mirror, the rest of the ray is reflected after refraction from the silvered surface, and follows a path parallel to that of the first reflected portion of the ray. In this way, you see, you have a duplicated image, and that is the drawback to any instrument that is dependent on this sort of arrangement of mirrors.

Prismatic Mirrors.

Now, when I began making these instruments, I started away first of all merrily making them up with monocular prism glasses, and similar attachments, and they were more or less satisfactory; but, on the whole, they were less so, and the reason is simply that you get duplication, or, rather, triplication of your image, and that interferes with the definition, especially when there is a great contrast between a light and dark object. Well, now, it occurred to me that I could get rid of this difficulty by working the mirror in the form of a prism, say, of 1° or 2° deviation. The effect of this departure from parallelism in the two surfaces of the mirrors is that the reflected light is thrown off from the first surface of the mirror, and absorbed in the dull black interior of the tube, and one single stronger image is reflected from the silvered surface of the prism. The effect is comparable to that of a rectangular solid prism. Of course, one loses a certain amount of light by the reflection from the first surface of the silvered mirror, but, on the other hand, you lose a certain amount of light by absorption in the solid prism. Well, there is a device for getting a single image from a silvered mirror which is, I think, worth pursuing.

I must apologise for keeping you so long. I think there is nothing else I have to say, but perhaps I might just recapitulate the points on which I think discussion might be desirable. In considering the construction of the trench periscope we have to discuss how much one ought to concede for portability, how much for shelter, how much for size of field, and how much for the obscurity of it as an object. (Applause.)

Discussion.

MAJOR WILLIAMS said that as nearly all the periscopes bought by the War Office were sent to him to be examined before they were approved, he thought he might be able to give a

few hints. An enormous number of periscopes had been submitted to the War Office; some were splendid, others were no good at all. There was a very large number of different patterns, and they wanted several different varieties. For example, they wanted one like they had seen that evening, which was very good for short ranges, but was hardly good enough for use at a range of four or five miles, where the officer in charge of a battery had to observe the fall of the shell. For that purpose something with a magnifying power is required. Perhaps he might be allowed to give a general summary of the designs that had been sent in. They had had an instrument of a tubular form like the one they had seen that evening. Then they had had a much smaller one of tubular form, which had a telescopic arrangement, giving a magnification of $1\frac{1}{2}$ diameters. Then they had had a large number consisting of two mirrors mounted in a tin box—more of those than of any other sort—and some with a wooden framework and mirrors mounted in wooden frames. They had also had much better ones, which contained the very best optical work in the shape of prisms and lenses, and gave a field of four degrees and a magnification of about ten. What the War Office really wanted was periscopes suitable for every possible condition of active service, and what they were doing at present was to consider every design submitted to them, and, if it looked at all promising, they would order a certain number and send them out to the front for trial. They hoped in time to know exactly what was wanted, and to get three or four standard types. Nearly all the periscopes sent in suffered from certain defects, and he would point out what those defects were. The first and principal one was in connection with the mirrors. He thought nine-tenths of the sample instruments that came in had very bad mirrors indeed. In a small periscope like the one Mr. Dixey had shown them, the quality of the

mirrors was not of vital importance, but what the War Office always tried to arrange for was to have a periscope which could be used with either a binocular or half a binocular. If one used a binocular it was necessary to have accurately-surfaced mirrors, and they found from experience that if ordinary plate glass mirrors were used very good results indeed could be obtained, although the difficulty of the double image had to be contended with. All the larger periscopes, at any rate, were tested with a binocular. There were some advertisements on the other side of the Thames, about half a mile away, and one of the tests for the periscope was to see if they could be read by means of a binocular. In that way they got periscopes which gave really very good results; it was possible to see very nearly as well through the periscope as through the binocular direct. The size of the mirrors depended entirely upon what it was to be used for. If it was to be used in a small tubular periscope, the mirror must be small. If the object was 3,000 or 4,000 yards away one could afford to have large mirrors. The size of the mirrors, of course, decided, more or less, the size of the field. The lower mirror should, if possible, be broad enough for use with both halves of a binocular. But from experience it was found rather difficult to arrange for that, as unless the mirror was absolutely flat, a double image was obtained, due to the mirror being untrue. With regard to the field of view in the ordinary periscope with rectangular mirrors, he maintained that one did not want a square field, but a fairly broad, and a not very high one; especially in a flat country. If the mirrors were mounted in such a way that they were strained, they became distorted, and the definition became bad. Then, as regarded the body of the instrument, they had had folding ones and hinged ones; they had had them in tin boxes, and they had had some built up of canvas and wood, with hinged mirror frames.

Each one had some good points, but none of them were perfect. It was very desirable that, if possible, the instrument should pack up small, but, on the other hand, it was very difficult to make a satisfactory one that would do so. With large ones it was necessary to have some sort of support, and that was especially the case if binoculars were to be used with them. A tripod stand was not wanted; probably the best thing was to have a pivot underneath the periscope from which one or two prongs projected. These could be stuck into the side of the trench, and the instrument could be revolved on the pivot and allow of the observer seeing in any required direction. One of the very best supports he had seen consists of a ball and socket joint and a screw which could be very easily screwed into a wooden baulk and buried in the ground. The next thing was that when the instruments came for inspection they were sometimes found to have a brilliantly enamelled surface which, when they were used in the trenches, would flash in the sun and give the whole situation away. What was done with such periscopes was to rub them over with turpentine and sand paper and make the surfaces as rough as possible. Dabs of different coloured paint were also put on them so as to make them as invisible as possible. Then as to the height between the mirrors: as Mr. Dixey had said, it was difficult to say what was the correct height, but he thought there was no doubt that between 18 inches and 2 feet was what was wanted at present, although sometimes smaller ones were very handy and convenient and would no doubt be used. It was very important, if used in the trenches near the enemy—which were sometimes within 50 or 100 yards—that they should be small, or they would be shot away at once. Then, if they were in built-up frames or consisted simply of two mirrors sliding on a bar, the top one must be rigid, because if it shook about in the wind it was

impossible to observe properly with it. Another very useful adjunct was to have a support for binoculars in front of the lower mirror, so that one could rest them on the support and get the object glass close up to the mirrors. Some useful periscopes allowed of the distance between the mirrors being varied; these had two mirrors, the top one probably fixed and the lower one capable of being slid up and down a rod. That was an advantage certainly, but no very satisfactory one of that type had yet been brought out. Instead of the mirrors being mounted in tin boxes, some mounted in wooden boxes, but personally he was very doubtful whether wooden boxes would stand much damp. One great drawback to some periscopes was that when a bullet struck the upper mirror the glass got shattered and a shower of pieces of glass came down the tube into the observer's eye. That was a serious thing, and he believed that some men had lost their eyesight in that way. If one used a binocular or half a binocular that would not happen. Then there was the question of having spare mirrors. He had never encouraged having spare mirrors, because most of the mirrors they had were $\frac{1}{2}$ in. thick, and it was very difficult to break them accidentally, and a bullet would probably break up the mirror frames. He must ask them to take all that he had said as his private opinion, and not as official in any way. He might mention that if anyone invented a periscope they should send it to the "Secretary, War Office," with full particulars about it. (Applause.)

MR. T. Y. BAKER said it seemed to him that the point raised about getting a good definition of the image and avoiding duplication was surely a question only of having mirrors of suitable parallel glass. The double reflection that had been spoken about, the reflection off the upper mirror and then off the lower one, was no different from the kind of reflection they got in a sextant where the parallelism of the

glasses was always good enough to stand a definition of something like eight or nine diameters. He did not know the relative expense, but it was perfectly possible to make mirrors of the size shown in Mr. Dixey's instrument at a very reasonable cost, and without any danger of getting duplication of images which would be produced by using a piece of ordinary plate glass. Mr. Dixey's method of avoiding duplication by making his upper mirror more or less prismatic was exceedingly ingenious, but of course it resulted in losing a certain amount of light, and that could be avoided by using mirrors made sufficiently accurate, such as sextant makers used for their sextant mirrors.

MR. WILLIAM SHACKLETON said there was great difficulty in procuring good sextant mirrors at a reasonable price at the present time, but there was still a large amount of good reflecting glass on the market. He had, in the course of his duty, to examine a large number of unworked mirrors for heliotrope—not heliograph—work, and he found about fifty per cent. of the mirrors selected would stand a power of about four times. The mirrors used were about $\frac{1}{2}$ in. thick. The prism method was used in solar eye-pieces, namely, in the solar diagonal, for getting rid of the double image.

MR. SMITH said there was one way of getting over the difficulty of the double image, although it was possibly a process which opticians did not regularly employ. Nevertheless, he believed it was perfectly easy; and that was to platinise, not silver, the front surface of the glass.

MR. H. F. PURSER said he had been about to suggest the same thing with regard to the surfacing of the mirror on the front. The method was used, as some of them were aware, for photographic mirrors, but it was not satisfactory from the point of view that it perished much more easily than the mirror with the surface on the back. He presumed for that reason alone

it would not be very satisfactory when used for periscopes.

MR. SMITH said it was not a question of silver, it was a question of platinising on the front surface.

MR. PURSER said there was also a method of silvering the front surface of the mirror, but from the point of view of durability he did not think that would be satisfactory. On the questions of portability and of the length of the periscope he believed it was practically not possible for them to standardise those things, because the varying conditions under which the instruments had to be used must necessitate, if satisfactory use was to be made of them, a variety of lengths and sizes, according to the distances which separated the trenches. What would be suitable in one case would not do at all in another case. Another thing was that the ground separating the two trenches was not always perfectly flat; sometimes it rose. That also would affect very seriously the length of the instrument.

MR. CHALMERS expressed the desire that those who had had some experience of these instruments would say whether there was any great disadvantage in a type which consisted of an open framework with mirrors top and bottom. It seemed to him that the making of a hole in the way described was a very cumbrous way of making those mirrors if there was no disadvantage in the other method.

MAJOR WILLIAMS said he did not know that there was any disadvantage about it; the only thing was that when looking into the lower mirror one saw the view reflected by the upper mirror surrounded by a view of the sky. That was not a very great disadvantage.

MR. CHALMERS asked whether there would be any objection from the point of view of the observer seeing by the aid of the mirror into the side of the trench?

MAJOR WILLIAMS said he did not think so; he

would only see the face, because the field would be so very little. Some of the instruments the War Office had received were built up on that principle.

MR. CHALMERS said he should have thought that there might be a certain amount of chromatic effect, and it would be necessary to take care that the other mirror was so exactly set that one cancelled the other.

MR. DIXEY said he would answer that at once. One would think that a comparatively deep prism was desirable, but a prism of one degree deviation was quite sufficient to carry the ray off, and the chromatic error caused by the ray going through the prism would be neutralised exactly by the other prism. In any case, the amount of chromatic error caused by a 1° prism was imperceptible. There was one other little technical point he would like to call attention to, if he might, in response to the observations made, and that was Major Williams' observation about the side images. Major Williams, he thought, had not accounted rightly for the side image sometimes obtained through the periscope. Obviously the two mirrors set in the way he had described were tilted, and the tilt of the mirror could always be resolved into a horizontal tilt and a vertical tilt. Whatever side-way tilt was given to any object, it could always be resolved into those two phases. The vertical tilt was important in a minor degree, because theoretically, in the simple arrangement he had shown the best position without high magnification or anything of that sort was that each mirror should be set at 45 degrees, and exactly parallel to each other. In virtue of the verticality of the tilt, it was impossible to avoid a ghost image, but the mirrors might have also a horizontal tilt, and whenever that was the case it would be seen, if they followed the diagram which he had made of the effect of the duplication of the image that a side image was necessary. It required most accurate adjustment to

get rid of those side reflections, but it was possible; and in the instrument he had shewn them it had been done; there was no side reflection there at all.

DR. CLAY said there was one point he would like to say a word upon, namely, the parallel mirrors. If one took an ordinary, fairly thick, piece of looking-glass, and held it up so that it reflected a distant lamp, almost at grazing incidence, one would see nearly always a series of images which would run off and get fainter and fainter. Everybody knew that, but everybody did not know that if one turned that piece round in its own plane, that series of images would turn round with it and make one revolution with one revolution of the mirror. The series of images obtained by looking at a distant image in that way was not due to the reflection from the front and back surfaces of a parallel plate of glass, according to the usual diagrams given in the text-books; it was due to the mirror not having the front and back surfaces quite parallel. If one repeated the experiment with a large ordinary sheet of plate-glass, and moved it about, one could often find a place where the images would come together, and if that place were marked and cut out it would prove to be a piece of glass of which the faces were accurately parallel. (Applause.)

THE PRESIDENT asked whether it would be of any practical value to have a little electric torch in the handle of the instrument, so as to be able to signal from the forward to the back trenches without the enemy being able to see. The advanced observers were frequently asked to give information to the rear as to what was happening, and it was not always possible, he believed, to have trench telephones. Another matter was the absolute necessity of guarding against damage to the eye from splintered glass. A piece of gauze might be used which need not obscure the vision, because by placing it close to the eye it would have but very little

blurring effect. Another subject closely akin to trench periscopes he might perhaps mention. Several soldiers had complained of severe stiffness of the neck owing to looking for aeroplanes; a number of people were told off every night in London for that duty, and all over the coasts watchers were gazing at the sky for hours. This was an excessively trying experience, and he did not see why a little dodge should not be used which he had employed some years ago. It seemed to him that it was quite possible to have such a sky periscope as would enable those watching for hostile aircraft to sit in a natural position. The vault of the sky could be very well studied by looking downwards at the surface of a mirror held in the hands.

ON TESTING ASTIGMATISM.

DISCUSSION INTRODUCED BY DR. R. S. CLAY,

4th February, 1915.

DR. REGINALD S. CLAY said: It is quite possible that the method which I am going to suggest for testing for astigmatism is known to a good many of you. It is one I have explained to people for some years, but I have never actually seen it in print anywhere, and I do not think it is as well known as it deserves to be. To make it quite clear, let me begin in this way. Supposing there is a hole in a piece of wood and a wooden peg that fits the hole. If both the peg and the hole are perfectly circular the peg will turn round smoothly, and will be no tighter in one part than another. Now, supposing that either the peg or the hole is not quite circular, but oval. For instance, suppose the peg is not quite circular. If the hole is perfectly circular, although the peg does not fit anywhere, it fits just as well in one position as in another, and it does not bind in one place more than in another. Or if the peg is circular and the hole oval; again it will not fit, but it will turn round smoothly in the oval hole, and it will not bind anywhere. But if both peg and hole are oval, the peg will bind twice and fit twice in each revolution.

That is the principle of a method that I have found exceedingly valuable in convincing people that they have slight astigmatism. Suppose a person has quite small astigmatism, say, one axis, the vertical axis is $\frac{1}{2}$ D. stronger than the horizontal axis; if that person looks at either a horizontal or a vertical line within his range of accommodation he can see it clearly, and if he is asked whether a horizontal or a vertical line is most indistinct, it is quite possible he

will say that he can see them both clearly. But if a weak cylindrical lens, say a $\frac{1}{2}$ D. or 1 D., is held in front of his eye and turned round in its own plane (he may be told to begin with that he will very likely find that the lens will make his vision worse wherever it is, and he is rather pleased if it does not), he is quite willing to confess that he can see better through the lens in one position than in another. It is obvious that if it is worse in one position than the other it is a case of the double oval; since a cylindrical lens combined with his eye can only produce worse vision in one position than in the other when the eye also is unsymmetrical. I have always succeeded in convincing a person by that experiment that he really has astigmatism. I had a case only last night. I noticed the eyes of a student there looked tired, and the lids inflamed, and I asked her whether she was troubled with headaches. She confessed that she was sometimes. I said, "Is there anything the matter with your eyes?" "Oh, no," she said, "there is nothing at all the matter with my eyes." I dare say if I had showed her an astigmatic chart she would have said she could see all the lines clearly. She could see them in the way she always had seen them. She may have always seen one of them a little hazy, and she is accustomed to a vertical line as a hazy one. Just as you associate a certain smell, a lemon, for instance, with a certain taste, so she associates a hazy line with a vertical line, and is not going to confess that she cannot see it clearly. But when I had put a cylindrical lens in front of her eye she agreed that the vision was much worse with the lens in one position, and I easily convinced her that her eyes were not symmetrical.

The method has this advantage—that the effect of the extra cylinder cannot be overcome by an effort of accommodation. If one meridian is already, let us say, a diopter stronger than another one, and a $\frac{1}{2}$ D. cylindrical lens is

added so as to increase the difference, no effort of accommodation can prevent it reducing the acuity. While if it is turned to the opposite axis the accommodation enables the patient to focus properly. Quite small type should be used (the ordinary reading test type), which will become sufficiently indistinct with the double cylinder to show the person the lowering in acuity produced. I have always found it quite easy first of all to see if there is slight astigmatism present, and, secondly, to convince the patients by an appeal to their common-sense, as they can appreciate that their eyes cannot be symmetrical if a rotation of the lens produces any change in their acuity.

On Methods of Sight-Testing.

I desire to offer two criticisms of ordinary sight-testing. First of all, I believe there are still a good many people who use the clock face chart for astigmatic testing. I have found persons who have argued that if the axis of astigmatism is along either of the groups of lines, then it is quite as good, perhaps better than, the fan test, and if the axis of astigmatism lies between two, then it can be located by the comparative indistinctness of the groups. I thought this was quite right until I happened to try it on myself, and I find with my right eye, by estimating the relative haziness of the two lines, that the axis of astigmatism is from 5 to 10 degrees away from the position that I get with the fan. I do not know how to explain it. I can only suggest that the difference is due to a physiological cause.

One other thing. Whilst I believe everyone who does sight testing tries, when he has obtained the approximate correction, whether there is any improvement with a plus or minus $\frac{1}{2}$ D. spherical lens, I am not sure that there is the same anxiety to get the astigmatism correct to the last $\frac{1}{2}$ D., and in my opinion the astigmatism correction is much the more im-

portant. If there is just a little astigmatism left the eye is probably continually accommodating backwards and forwards, as if it looks at a horizontal line it sees that best with one accommodation, and if next to it there is a vertical line it sees that with a slightly different accommodative effort, and as a result the person undoubtedly gets a headache. So that, really, even more care should be exercised in obtaining the correct cylinder than the correct sphere. I therefore suggest that after the approximate correction has been found, a weak cylinder should invariably be held in front and turned round; if there is still a slight cylindrical error uncorrected, when the cylinder adds to that error the patient's acuity will be lowered as compared with the acuity obtained when the cylinder is rotated through a right angle, and it will indicate that there is still some astigmatism left uncorrected.

Discussion.

MR. F. W. DADD said he had not tried Dr. Clay's method himself. He thought opticians generally really did try to correct the smallest amount of astigmatism, and, having obtained the approximate amount by one method, usually tried the addition of weak plus or minus cylinders to see if any improvement in visual acuity could be made. For his own usual method of testing he always tried to keep the eye being tested back in its vision until what he considered to be the whole of the astigmatic error was corrected, viz., to keep the accommodation suspended until the astigmatism was cleared up.

MR. H. FIRTH said there was one point which had never yet been threshed out, and that was whether it was possible to accommodate for astigmatism. Could the ciliary muscle receive innervation in certain meridians so that an astigmatism of the crystalline lens would be produced and so neutralise that of the cornea? If the acuity was not very good it was quite pos-

sible that the patient would accommodate for the circle of least confusion, which, after all, was the nearest approximation to a point image in the astigmatic pencil, and in that case all the lines of a fan or the letters of a test chart, would be seen equally indistinct. Therefore, as a preliminary test for astigmatism, he did not see exactly how the rotation of a cylindrical lens was going to help. On the other hand, as a final test in cases of good acuity, he thought the rotation of a very weak plus cylinder, or, better still, a "Jackson" cross-cylinder (plus in one meridian and minus in the other), would certainly help them to arrive at an exact astigmatic finding.

MR. W. A. DIXEY said one point had struck him as rather interesting in listening to Dr. Clay. Everybody was familiar with the history of fatigue in connection with slight astigmatism, and it was quite possible (he had not seen it suggested before) that it might be due to the point Dr. Clay had suggested, namely, the constant operation of the accommodation in accommodating first to horizontal and then to vertical lines. It was a little fanciful, perhaps, but there was something in it.

THE PRESIDENT: It has frequently been suggested.

MR. DIXEY said he had not seen it. At any rate, the two directions were at right angles to each other, and it did not matter whether they were horizontal exactly; there had been variations in accommodation in any case.

THE PRESIDENT said that the method described by Dr. Clay was old wine in a new bottle, if he might say so. The oldest and perhaps the best clinical method of estimating astigmatism quickly was trial rotation of cylinders. When one asked a person to tell the difference in distinctness between the lines at right angles to each other, one was asking their opinion, and a person's opinion was often very much at fault, more especially if he was not accustomed to

scientific observation. It was very much easier to test, quickly and accurately, in the case of a person accustomed to scientific observation than in the case of a person who had never learned the meaning of the term. He thought, therefore, they should eliminate matters of opinion as much as possible. They could get what was required by ascertaining the smallest size of type that could be read. That was why one passed beyond the 6/6 standard in testing. If a person had a low degree of astigmatism something was sacrificed in the visual acuity—it might be one per cent., two per cent.: it did not matter—something had gone; the astigmatism could not be diagnosed, and need not be corrected unless there was this shortcoming. Let them assume that the case was one of an extremely low degree of astigmatism—say, an error of $\frac{1}{4}$ D. That person could read 6/6 but not 6/5. They would “rotate their oval peg over their oval hole” until they found it fitted exactly; when the meridians were in alignment and co-axial the patient could read 6/5, but when at right angles 6/6 was blurred, because they had doubled his error. So that one did not ask for the patient's opinion; one simply took them to the extreme limit of vision needed and then ascertained whether the cylinder would extend it. They were tested on a number of lines all in different directions, these lines constituting the integers of letters. Another point which arose was about the relation of astigmatic and spherical error. He quite agreed as a general principle that the astigmatic error was the one which would cause most trouble, but could not make a golden rule about it, because it must exist in certain proportions. If the spherical error was great, for instance, 3 degrees of hypermetropia, with half a diopter of astigmatism, it was the spherical error that was worrying the person. One could not lay down a law, but he agreed that if one of the two had to be dispensed with, as a rule

one would go for the correction of the astigmatism. Finally, the old vexed question of sectional accommodation had come to the surface again, and one could only surmise about that. One observer said that he had worn weak concave cylinders all day long and for several days, and he had never succeeded in making the lines of the star chart equal, which he would have done had he been able to exercise any sectional accommodation at all. But then they were surrounded with clinical evidence, for instance, that people of low degrees of astigmatism suffered much more from asthenopia than people with high degrees. How was that to be explained otherwise than that they were struggling to overcome it? The only trouble with regard to sectional accommodation was that there was no definite proof that it really had taken place.

INSTRUMENTS FOR THE MEASUREMENT OF SOLAR RADIATION.

BY MR. ROBERT S. WHIPPLE, M.I.E.E.

Read 11th March, 1915.

THE measurement of the amount of energy received by the earth from the sun must always be of interest to mankind. Little excuse, therefore, need be made for bringing before the Optical Society a short account of the instruments now in general use for this purpose, more especially as very marked advances have recently been made in the design and construction of apparatus for the measurement of solar radiation.

Two factors are of importance:—

- (1) The intensity of the radiation received from the sun, and
- (2) The duration of sunshine.

The latter factor is the one that appeals to the man in the street, although in reality the former is of far more importance to his welfare.

For many years meteorologists have endeavoured to record the number of hours during the day on which unclouded sunshine has fallen on the earth's surface, and several instruments have been designed for this purpose. The one that has been most generally used is a modified form of that designed by J. F. Campbell, of Islay, in 1853, and described by him in the Report to the Council of the British Meteorological Society for 1857.¹ The original instrument consisted of a spherical glass bottle filled with acidulated water and

¹On a New Self-registering Sun Dial, J. F. Campbell, Report of the Council of the British Meteorological Society, May, 1857, p. 18.

supported centrally in a white stone bowl, the globe being about 6 ins. in diameter. The bowl was engraved with hour lines, and its surface blackened with an oil paint or varnish. The bottle acted as a burning lens and the sun's heat melted the paint wherever the rays impinged upon it. The observer read off the duration of sunshine for each hour and marked it on a paper ruled for the purpose. Campbell, in his paper, recommended the employment of a wooden bowl, and also suggested the use of photographic paper as a means of re-

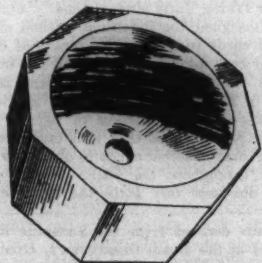


FIG. 1.—Sunshine Record obtained on the interior of a Burnt Wooden Bowl.

ording the duration of moonshine. A solid glass sphere was afterwards substituted for the glass bottle filled with water, and was mounted in the centre of a wooden bowl. The record (fig. 1) was obtained upon the northern side of the interior of such a bowl, the wood of which became more or less deeply charred by the sun's rays, according to the amount of sunshine experienced during the period.

In 1876 a sunshine receiver, in which a metal bowl was employed instead of a wooden one, was mounted in Greenwich Observatory,

and cards to receive the sunshine were held in position by means of clips. In the report dealing with this work² mention is made of experiments tried with various coloured cards, and it is stated that blackened millboard was found to be the best for securing the early morning and late evening sunshine. R. H. Scott had suggested³ the employment of cards on which to receive the sunshine, but he appears to have been forestalled by Campbell, who had made the experiment in 1857, and who, in referring to it, said:⁴ "the strip was the first plan tried, and but for the trouble, it is the best for taking daily observations." Stokes, in 1879,⁵ designed the ingenious method of holding the cards in position by means of slots in a metal bowl partly surrounding the ball. A modern Campbell-Stokes instrument is illustrated in fig. 2, and the slots for holding the cards are shown in section in fig. 3. These slots are so arranged that it is only necessary to have three different cards to obtain sunshine records throughout the year. In the paper, Stokes discusses very fully the effect of lati-

²Results derived from the Sunshine Records obtained at the Royal Observatory, Greenwich, by means of Campbell's Self-registering Sun Dial during the year ending April 30th, 1877, by William Ellis, *Quarterly Journal of the Meteorological Society*, Vol. III. 1875-77, p. 460.

³On the Registration of Sunshine, by Robert H. Scott. *Quarterly Journal of the Meteorological Society*, Vol. III. 1875-77, p. 18.

⁴See "Sunshine Recorders and their Indications," by Richard H. Curtis. *Quarterly Journal of the Royal Meteorological Society*, Vol. XXIV. p. 3.

⁵Description of the Card Supporter for Sunshine Recorders adopted at the Meteorological Office, by G. G. Stokes. *Quarterly Journal of the Meteorological Society*, Vol. VI., p. 83.

tude on the position of the receiving bowl, and the reasons why blue cards were finally decided upon.⁶ He also gives full instructions for adjusting and using the instrument.

As originally designed, the Campbell-Stokes Recorder was constructed for the particular latitude only in which it was to be employed. Various models have been constructed to overcome this difficulty, one of the best known being that due to R. H. Curtis⁷ (shown in fig. 4).

⁶The reasons given for the choice of the blue card are of interest; after discussing the experiments with darkened cards, Stokes remarks, "The difference between different kinds of cards was, however, far less than might perhaps have been anticipated. It was only in catching a few minutes more or less of a very feeble sunshine that, with the exception, perhaps, of a few pale and unsuitable kinds, one card differed from another. Cards darkened to a grey with carbon were among the best. It was decided, however, ultimately, to employ Prussian blue only moderately dark. Such a card when viewed through a red glass, which transmits all the visible rays which are strongest in heating effect, looks almost black, while the rays of high refrangibility which it freely reflects enable the record to be easily seen, while entailing but little loss of absorption of rays powerful in their heating effect. It is needless to remark that if a pigment were chosen *merely* from '*a priori*' considerations, its behaviour with respect to the invisible rays lying beyond the red would have to be taken into consideration. But to do this experimentally would involve an expenditure of time which the value of the result would hardly justify, since the suitability of a pigment may be ascertained by direct trial."

⁷"Sunshine Recorders and their Indications." by Richard H. Curtis. *Quarterly Journal of the Royal Meteorological Society*, Vol. XXIV., p. 17.

In this instrument the position of the bowl relatively to the sphere can be moved through several degrees of arc. (The position of the sphere can also be varied in order that it may be centred correctly relatively to the bowl.) In the tropics the bowl must be placed more or less under the sphere, and in this case the sphere is supported by gripping it between two supports placed in the direction of the polar axis, as shown in fig. 5. Another form of mounting is also em-

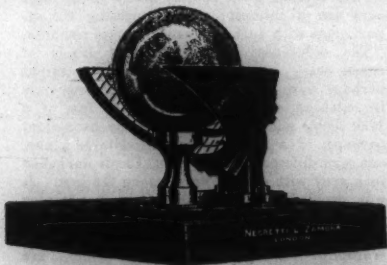


FIG. 2.—Campbell-Stokes Sunshine Recorder.

ployed when the instruments are used in either the Arctic or Antarctic regions.⁸

A detailed specification of the Campbell-Stokes' instrument is given in the useful handbook published by the Meteorological Office, together with instructions for its adjustment.⁹

Another modified form of the instrument (fig. 6) due to the late Superintendent of the

⁸See National Antarctic Expedition, 1901. "Meteorology," Part I., page 512. Royal Society, London.

⁹"The Observer's Handbook." M.O. 191 (1914). Meteorological Office, London, pp. 83-94.

Kew Observatory,¹⁰ allows of the recorder being used in any latitude, and also permits of straight cards being employed instead of the



FIG. 3.—Slots in Recorder.

circular ones used in the Campbell-Stokes Recorder.

The Jordan Sunshine Recorder (see fig. 7), in-

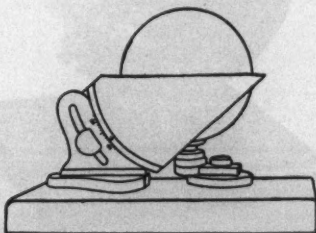


FIG. 4.—Diagram showing Curtis' modification of Campbell-Stokes Recorder.

¹⁰ Whipple-Casella Sunshine Recorder. This instrument was first exhibited at an Exhibition of Sunshine Recorders held by the Royal Meteorological Society in 1885. The catalogue of this exhibition contains an interesting description of many old forms of recorders and actinometers. See Quarterly Journal of the Royal Meteorological Society, Vol. XI., p 242.

vented by James B. Jordan¹¹—in reality an improved form of the instrument invented by his father, T. B. Jordan—records the varying intensities of sunshine by altering the amount of discoloration produced in a paper sensitised by the ferro-cyanide process. A strip of prepared paper is put into a brass box, and the sun's light allowed to pass through a small slit in the

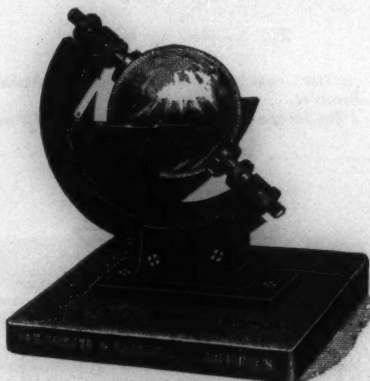


FIG. 5.—Campbell-Stokes Sunshine Recorder
(Equatorial Pattern).

side of the box, and to fall upon the paper. After exposure for the day, the paper is fixed by immersion in clear water, when a blue trace

¹¹Notes as to the Principle and Working of Jordan's Improved Photographic Sunshine Recorder, by James B. Jordan and Frederic Gaster. Quarterly Journal of the Royal Meteorological Society, Vol. XII. 1886, p. 21.

will be found upon its surface which roughly varies with the intensity of solar radiation.

As regards the records obtained with the Campbell-Stokes and the Jordan instruments, it will be at once recognised that there is a great difference between them. In the one case



FIG. 6.—Whipple-Casella Recorder.

the record is calorific, in the other actinic. In 1896-7, R. H. Curtis, on behalf of the Council of the Royal Meteorological Society, undertook a careful examination of a year's records obtained from two of these in-

struments.¹² He found that, as a whole, the records agreed very well in the number of hours' sunshine recorded, but that the measurements of the Jordan curves are open to considerably more uncertainty than are measurements of the Campbell-Stokes curves. In his paper dealing with the subject, Curtis makes some useful suggestions as to the definition of "bright sunshine" and discusses fully the mounting of sunshine recorders.

W. Marten, of Potsdam Observatory, has also

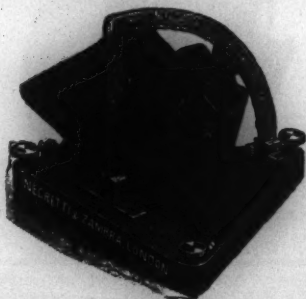


FIG. 7.—Jordan Sunshine Recorder.

made a comparison between the Campbell-Stokes and Jordan instruments, and is in agreement with R. H. Curtis. He states¹³ that the Campbell-Stokes instrument registers earlier in the morning and later in the evening than the Jordan instrument, and is thus more suit-

¹²"Sunshine Recorders and their Indications." Ibid. pp. 16-20.

¹³Ergebnisse zehnjähriger Sonnenscheinregistrierungen in Potsdam, W. Marten, Ergebnisse der meteor. Beob. in Potsdam, 1904.

able for winter months, also the records are easier to interpret and more permanent. Moreover the sensitivity of the Jordan instrument depends on the quality of the photographic paper. On the other hand, the Campbell-Stokes instrument is apt to over-record at mid-day, and is not so accurate when the sky is clouded. The cards are also liable to be spoilt by rain, but yet it is to be preferred as the more accurate of the two instruments.

The United States Weather Bureau has

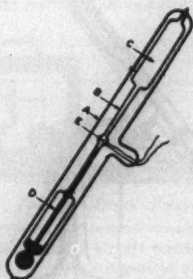


FIG. 8.—Section of Marvin Sunshine Recorder.

adopted the Marvin Thermographic Sunshine Recorder as the official instrument for recording the duration of sunshine. It is essentially a differential air thermometer of the type invented by Leslie and afterwards adopted by several other workers.

A section of the thermometric part of the apparatus is shown in fig. 8. It consists of a straight glass tube with cylindrical bulbs, C and D, one at each end, bulb D being smoothly

coated on the outside with lampblack, and the whole enclosed in a protecting glass sheath A. The space between the inner tube with its bulbs and the protecting sheath is first thoroughly dried, then exhausted of air and hermetically sealed. Mercury is used to separate the air in the bulbs, a small quantity with a little alcohol being placed in the bottom and stem of the lower blackened bulb D. A pair of electrical con-

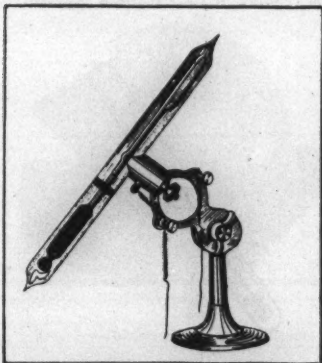


FIG. 9.—Marvin Sunshine Recorder.

tacts is fitted at E. The instrument is mounted on a support, as shown in fig. 9. The black bulb of the instrument is held downwards. When the black bulb is heated by radiation falling upon it, the mercury rises and closes an electric circuit between the contacts, this circuit actuating a recording counter. The inclination of the recorder is so adjusted that the mercury column keeps the circuit closed during times when the disc of the sun can just

be faintly seen through the clouds. Full instructions for using the instrument are published by the Weather Bureau.¹⁴ The author understands that there are a large number of these instruments in use in the United States.

W. H. Dines invented¹⁵ a somewhat similar instrument (see fig. 10), in which a black bulb ether-filled thermometer was mounted on pivots and tipped over whenever the radiation exceeded a determined amount. The movement was caused by a thread of mercury which, when moved by the expansion of the ether in the black bulb, disturbed the equilibrium of the system, the mercury falling against electric con-

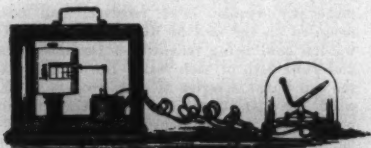


FIG. 10.—Dines' Recorder.

tacts, completing a circuit through a form of chronograph. By an ingenious arrangement of contacts in the recording mechanism, no current is used whilst "sunshine" or "non-sunshine" is being recorded. It is stated that so little current is required to operate the instrument that "a single Leclanché cell, value

¹⁴Instructions for the care and management of Electrical Sunshine Recorders (Weather Bureau pattern), U.S. Department of Agriculture, Weather Bureau, Circular G, W.B. No. 518, 1914.

¹⁵The Ether Sunshine Recorder, by W. H. Dines. Quarterly Journal of the Royal Meteorological Society, Vol. XXVI., p. 243, 1900.

about one shilling, will work the apparatus for two or three years."

It will be observed that duration of sunshine only is recorded in both these instruments, although to do so the radiation must have a definite intensity. Even with the Campbell-Stokes and Jordan instruments it is possible to make a rough comparison of the intensity and duration of the sunshine on any two days, yet not practicable to convert the values obtained into absolute units of radiation.

An attempt to compare the relative intensity of one year's sunshine with another was made by H. E. Roscoe and Balfour Stewart.¹⁶ For some years a Campbell glass ball recorder, with mahogany wooden bowl receiver, had been mounted on the roof of Kew Observatory, the wooden bowl being renewed on December 21st and June 24th of each year. During the year the sun burnt a series of deep recesses in the wood, the quantity of wood burnt being roughly proportional to the intensity of sunshine. (See fig. 1.) Balfour Stewart used an ingenious method for determining the weight of wood burnt, and showed that the quantity burnt varied with the eleven-yearly periods of sun-spots, the maximum intensity corresponding with the minimum sunspot period.

Before describing any of the more recent instruments employed for the measurement of solar radiation, it will be well to consider the problem briefly from the theoretical point of view. The solar physicist wishes to determine the quantity of heat received by the earth from the sun and the rate at which it is received. This quantity, which is so important in all Solar Physics work, is called the *Solar Constant*, and is defined as follows:—

¹⁶On the Heat of Sunshine at London during the twenty years 1855 to 1874, as registered by Campbell's method, by H. E. Roscoe and B. Stewart. Proceedings of the Royal Society, No. 163. 1875, p. 578.

"The quantity of heat received in one minute from the sun at his mean distance from the earth, by one square centimeter of a perfectly absorbing surface presented normally towards the sun, and supposed to be situated just outside the earth's atmosphere, is known as the *Solar Constant*."¹⁷

It is probable that this quantity is not really constant, as most likely the various portions of the sun's surface presented to the earth have different radiating powers, and there may be also a periodical variation corresponding to the variation in the area of sunspots.

A glance at the definition of the Solar Constant shows the one outstanding difficulty of the problem, viz., the impossibility of measuring the radiation received just outside the earth's atmosphere. The absorption by the atmosphere and the water vapour it contains is very great, and is, of course, a varying quantity.

It depends on the amount of dust and moisture in the air, on the height of the observing station above sea-level, and on the elevation of the sun above the horizon. The length of the path of sunlight between two horizontal layers is proportional to the secant of the zenith distance of the sun, and, therefore, the absorption may be expected to be an exponential function of this secant. From observations taken at different hours of the day the nature of this function can be determined, and the strength of the radiation falling on the outermost layer of the atmosphere can be estimated.¹⁸

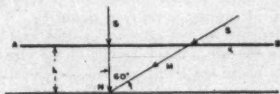
¹⁷The Theory of Heat, Thomas Preston, 2nd Ed., p. 616. See also a text-book of Physics—Heat, J. H. Poynting and J. J. Thomson, 4th Ed., p. 251, and Rapports presentes au Congrès International de Physique, Vol. III., p. 453, 1900.

¹⁸It may be of interest if this statement is illustrated by the consideration of a simplified problem.

It is now realised that the only way to obtain accurate data of the solar constant is by a bolometric examination of the solar spectrum, and this can only be carried out in an observatory with an elaborate equipment. The bolometric examination is necessary not only because some rays are completely absorbed, but because the co-efficients of absorption of those waves which do reach the earth depend upon their wave-lengths.

The first successful research on the Solar Con-

Let S represent the radiation from the sun reaching AB , the upper limit of the atmosphere, which for simplicity is regarded as homogeneous and of depth h , and let it be assumed that the observations are taken in



the tropics. Let N be the amount of radiation reaching the earth when the sun is overhead, and M the amount when the zenith distance of the sun is 60° . In the first case the rays traverse a path of length h , and in the second case a length of $2h$. If the falling off in the strength of radiation is expressed by the factor K , then $N=KS$. In the case of the oblique ray the intensity half way to the surface will also be KS , and the strength will fall off in the same ratio for the second half of the path, so that $M=K^2S$. It follows that

$$K = \frac{M}{N} \text{ and } S = \frac{N^2}{M}$$

stant was that made in 1837 by Pouillet,¹⁹ who designed a simple piece of apparatus for measuring solar radiation. This instrument, which he named the "Pyrheliometer," as shown diagrammatically in fig. 11. It consists of a flat thin cylindrical vessel A, of which the upper face is lampblacked and the rest silvered. This contains water, and serves as a calorimeter. It is mounted at one end of an axis D round which it can be rotated to secure mixing of the contents, and the thermometer stem lies along the axis, so that the temperature of the calorimeter can be easily read. The part A of the

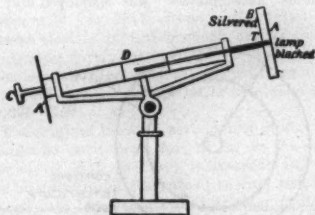


FIG. 11.—Pouillet's Pyrliometer.

instrument was first directed to a part of the sky away from the sun, and the fall in temperature noted during five minutes. It was then

¹⁹An excellent bibliography of the early work in the measurement of solar radiation will be found in the "Report on Radiation," by Jules Violle, to the International Meteorological Committee at St. Petersburg, 1899. M.O. 148. Appendix VIII., p. 42. H.M. Stationery Office, London. Mention should also be made of "Actinométrie," by R. Radau, Gauthier-Villars, Paris, as containing a good summary of early work on the subject.

directed towards the sun for five minutes, and the rise noted; it was finally directed away from the sun, and the fall noted during another five minutes. Knowing the capacity of the calorimeter, the total heat received per unit of surface per unit of time could be determined. The results obtained by Pouillet were remarkably good when one considers that at the time very little was known of the laws of radiation and of the absorption by water vapour. His value, 1.7 calories per minute per square cm., compares very favourably with the modern value, 1.93 calories.²⁰

Violle's Actinometer²¹ was employed for a

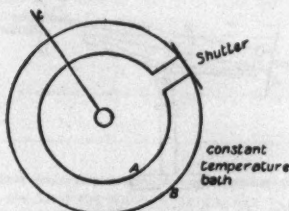


FIG. 12.—Viole's Actinometer.

considerable time on the Continent, it being more rapid in action than the pyrliometer of Pouillet. It consisted of a thermometer with a spherical bulb (see fig. 12), placed in the middle of a double-walled enclosure (internal diameter 150 mm.), the space between the walls being filled with melting ice or with water main-

²⁰Annals of the Astrophysical Observatory of the Smithsonian Institution, Vol. III., C. G. Abbot, F. E. Fowle, and L. B. Aldrich, p. 166.

²¹Report on Radiation by Jules Violle, Ibid. p. 47.

tained at a constant temperature. An opening provided with a shutter allowed the sun's rays to fall on the thermometer bulb. The shutter being closed, the thermometer reached the temperature of the enclosure, this temperature being noted. The shutter was then opened for fifteen minutes, after which time a stationary temperature was reached, the heat from the sun having balanced that lost to the enclosure. The water equivalent of the thermometer being known, the heat gained from the sun was determined. Some observations made by Mr. Joseph Vallot, in 1887, from the summit of Mont Blanc, with a Violle actinometer, gave a value of 2.0 calories per minute for the Solar constant.²² In 1879, Rosetti, by means of a thermo-electric pile of bismuth and antimony, made some careful experiments to determine the laws of radiation and the temperature of the sun. He obtained for the latter a value of about 10,000° C.²³

The original Herschel actinometer (about 1825) should be mentioned, for, as far as the author knows, it was the first instrument in which the observation of the rate of cooling was introduced into the measurement of solar radiation. The actinometer consisted of a large cylindrical thermometer bulb, with a scale considerably lengthened, so that minute changes could be easily seen. The bulb was of transparent glass, filled with a deep blue liquid, which expanded when exposed to the sun's rays. When an observation was to be taken, the actinometer was placed in the shade for one minute and read; it was then exposed for one minute to sunshine, and its indication recorded; it was finally restored to the shade and its reading again noted. The mean of the two readings in the shade, subtracted

²²Report on Radiation, by Jules Violle, *Ibid.*, p. 49.

²³Heat, J. H. Poynting and J. J. Thomson, *Ibid.* p. 255.

from that in the sun, indicated the amount of expansion of the liquid produced by the sun's rays in one minute of time. The Crova actinometer is a somewhat similar instrument, and is observed in a similar manner. It consists of an alcohol thermometer with an index of mercury; the bulb (20 mm. in diameter) is blackened, and receives the solar radiation through an aperture 10 mm. in diameter in a surrounding metal envelope. Diaphragms are provided to prevent the effect of wind, and to ensure that the solar beam falls normally on the bulb. A large number of observations were made with this instrument, but the values obtained for the solar constant were considerably higher than the modern values.²⁴ It is to be regretted that more experiments have not been made with actinometers of this type.

The preceding instruments for measuring the intensity of solar radiation are not now in general use, but are of considerable interest historically.

The following is an attempt to group the instruments now in general use under divisions depending on the physical properties employed for the measurement of the radiation. It can only be considered as a comparatively rough grouping.²⁵

(1) The generation of an electric current in a circuit composed of two dissimilar metals by the radiation which falls on one or more junctions, as in the thermo-pile of Nobili (often called the Melloni thermo-pile). The actinometer and radiation pyrometers of Féry, and the multiple piles of Rubens, Paschen, Coblentz,

²⁴References to the original papers by Crova and to work carried out with the instrument will be found in the "Report on Radiation" by J. Violle, *Ibid.* 50-52.

²⁵See also "The Measurement of High Temperatures," by G. K. Burgess and H. Le Chatelier. 3rd Ed., p. 267.

etc. The radio-micrometer of Boys may be included also under this heading.

(2) The increase in resistance of a metallic strip forming one or more arms of a Wheatstone bridge due to the rise in temperature caused by the incident radiation; such are the bolometers of Langley and Callendar.

(3) The equal heating of two metallic strips, one by the solar radiation falling upon it, the other by means of an electric current, the equality of heating being indicated by means of thermo-couples. Such an instrument is the Angström pyrheliometer.

(4) The measurement of the rise in temperature of a mass of metal or of a liquid in a given time. There are many forms of such instruments, more especially the earlier types, the best modern instruments being the Michelson and the Abbott silver-disc pyrheliometers.

(5) The employment of a black-body receiver combined with a water-flow calorimeter, as in the standard pyrheliometers of Abbott and Fowle.

(6) The balancing of the heat absorbed by a disc or cup by the peltier absorption of heat in a thermo-junction or junctions through which a measured electric current is passed. Such instruments are the radio-balances of Callendar.

(7) The deflection, or rotation, by radiation of vanes delicately mounted in vacuo, such as the radiometer of Crookes.

As previously stated, this grouping cannot be considered an absolutely comprehensive one, but the majority of instruments in general use may be placed in one or other of the groups.

Although the original thermopile of Melloni has been developed into an instrument of wonderful sensitivity by Rubens,²⁶ Paschen,²⁷ and

²⁶Ueber eine neue Thermoskule, von Heinrich Rubens. Zeitschrift für Instrumentenkunde, Vol. XVIII., p. 65, 1898.

²⁷Zur Kenntnis ultraroter Linienspektra

Coblentz,²⁸ yet, as far as the author is aware, the multi-thermopile has not been adopted in any form of pyrhelimeter in general use at the present time. It is not easy to understand why such is the case, unless it is that the old thermopiles had too great a lag for radiometric work, and the new piles are too sensitive and perhaps not sufficiently robust for the comparatively rough handling they would meet with in practice. They possess also the disadvantage that a sensitive galvanometer is required, and this practically prevents the employment of the instrument in the open air.

It may perhaps be of interest to give a brief description of the Paschen and Coblentz piles. Both of them are modifications of the Rubens pile, which in its turn was a modification of that of Melloni. In both piles sensitivity has been obtained by reducing the mass, and thus the thermal capacity, and by the use of thin wires the thermal conductivity between the hot and cold junctions has been lowered. The Paschen pile, illustrated in fig. 13, is of the multiple type, the mass being reduced by rolling the strips as thin as possible (0.02 mm.). In the illustration the pile is shown mounted in a tube fitted with a large number of diaphragms. An adjustable slit is mounted in front of the pile. Experiments show that when used in conjunction with a Paschen galvanometer with a period of about 7 secs. a deflection of 1 mm. is given with a change of 0.25° C. in the temperature of a body at a metre.

III., von F. Paschen. *Annalen der Physik*, Vol. 33, p. 736, 1910.

²⁸Instruments and Methods used in Radiometry, I. & II., and Various Modifications of Bismuth-silver Thermopiles having a continuous absorbing surface, by W. W. Coblentz. *Bulletin of Bureau of Standards*, Washington, Vol. 4, p. 391, 1908; Vol. 9, p. 7, 1911, and Vol. 11, p. 131, 1914.

The Coblentz pile (fig. 14), the elements of which are bismuth and silver, is even more sensitive than the Paschen, and is extremely rapid in action and remarkably constant in its readings. The pile consists of twenty junctions arranged in the receiver so that the junctions lie in a straight line, and slightly one behind the other so that it is impossible for radiation to pass through the pile. The receiving area is approximately 12×1.3 mm. As in the Rubens and Paschen piles, the cold junctions occur alternately on either side of the hot junctions, and are protected from external radia-

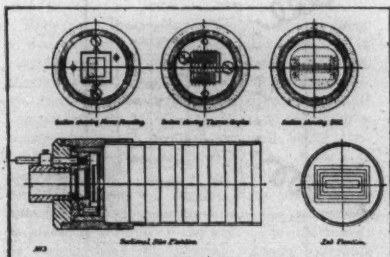


FIG. 13.—Paschen Pile.

tion. (In the figure the cold junction protecting plate has been removed.) Both of these piles are of great value in spectroscopic work, although as far as the author knows they are not being used in the measurement of total solar radiation.

The Fery pyrometer was the first commercial instrument to make use of total radiation as a method of measuring high temperatures. A very large number of these instruments are now employed in this manner but, as far as the author is aware, only one has been seriously employed

for the measurement of the sun's temperature, and thus the solar constant. The results obtained with the Féry radiation pyrometer are deduced by means of the Stefan-Boltzman law. *The total energy radiated by a black body²⁹ is proportional to the fourth power of the absolute temperature.*

This law has been confirmed experimentally

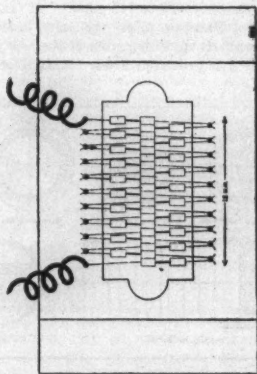


FIG. 14.—Coblentz Pile.

over the temperatures met with industrially. Féry designed a special pyrometer (see fig.

²⁹In a paper entitled "Radiation in the Solar System; its effect on Temperature and its Pressure on Small Bodies." Phil. Trans., Vol. ccii. A, p. 525, Prof. J. H. Poynting suggests the term "full radiator" instead of "black body," owing to the incongruity of applying the latter expression to luminous bodies (see also "Theory of Heat," Ibid., p. 619).

15) for the measurement of the sun's temperature.³⁰ By means of a concave mirror, one meter in focus and 103 mm. diameter, the heat rays are concentrated on to a small thermocouple 0.5 mm. diameter, the solar image being 8 mm. in diameter. The instrument was calibrated upon an electric furnace, and upon the crater of an arc, and the deflections were found to be proportional to the fourth power of the absolute temperature between 1600°. Absolute and the temperature of the arc. A large number of observations made with this instrument from the summit of Mont Blanc have placed the sun's temperature at 5360° C.

Féry has also developed a more simple form of actinometer, which has the advantage that it may be standardised directly electrically.³¹ A thermo-couple formed of the brass and constantan supports LK (fig. 16), supports a small copper receiver C, on the exterior of which a manganin coil is wound, the coil being connected to the terminals *bb'*. The tube *t* is sighted towards the sun, the radiation falling on the receiver. A window V is provided so that the observer may know that the instrument is correctly sighted on the sun. The instrument is standardised by passing known currents through the manganin coil, and observing the

³⁰Contribution à l'étude de l'émission calorifique du Soleil, by MM. C. Féry et G. Millochau, 'Comptes Rendus,' April 28th, 1902, and October 8th, 1906. Contribution à l'étude du rayonnement calorifique solaire, by MM. C. Féry and G. Millochau 'Comptes Rendus,' 3rd Feb., 1908. Note on the Solar Constant and the Apparent Temperature of the Sun, by M. C. Féry, Monthly Notices of the Royal Astronomical Society, Vol. LXIX., No. 7.

³¹Contribution à l'étude du rayonnement calorifique du Soleil, by MM. G. Millochau et C. Féry, 'Comptes Rendus,' Feb. 17th, 1908.

temperature rise in consequence. This instrument has the great drawback that owing to the mass of the receiver an observation occupies

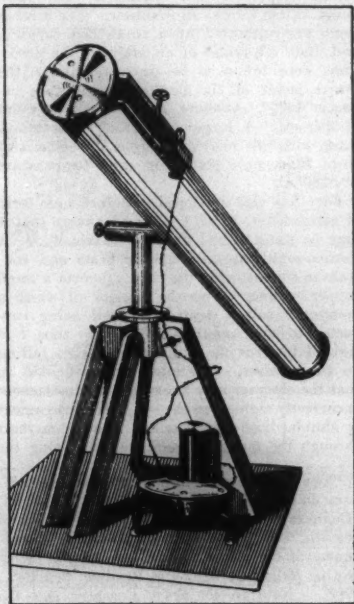


FIG. 15.—Féry's Solar Pyrometer.

twenty-three minutes, and thus it cannot be considered a practical piece of apparatus.

The Boys' radio-micrometer³² (see fig. 17) consists of an electric circuit, composed of a single loop of fine bare copper wire to the lower end of which a pair of very light bars of antimony and bismuth are soldered. These bars form a thermo-electric couple, and the circuit is completed by soldering them side by side at their lower ends, to a small disc of thin silver. When radiation falls on this disc the lower junction of the couple becomes heated and a current traverses the circuit, and in order to detect this the circuit is suspended in a strong

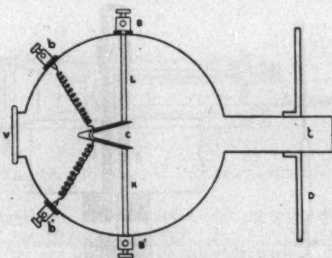


FIG. 16.—Féry's Actinometer.

magnetic field, in which it is deflected. The couple is protected from outside electric-thermal and magnetic influences by brass and iron shields. In addition to its great sensitiveness the instrument has the advantage of extreme rapidity of action. Wilson and Gray³³ com-

³²See The Radio-micrometer, C. V. Boys, Phil. Trans., Vol. clxxx. A., p. 159, 1888, and also "Theory of Heat," Preston, Ibid., p. 577.

³³"Temperature of the Sun," Wilson & Gray, Phil. Trans. A. 1894, p. 361.

pared directly the radiation from the sun with the radiation received from a constant temperature enclosure or a "black body," the temperature of which was known. The radiations from the two hot bodies were received on two couples in the same circuit of a Boys' radio-micrometer, the couples being arranged so as to differentially oppose each other. In this way a direct comparison of the relative intensities of the two radiations was received. The radiation from the sun was reflected into the instrument by a mirror of known reflecting power.

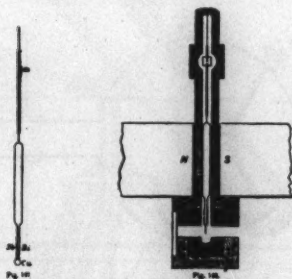


FIG. 17.—Boys' Radio-micrometer.

They determined the temperature of the sun as being between 6200°C . and 7400°C .

The bolometer, invented by Langley³⁴ in 1880, was designed to measure the distribution of heat in the solar spectrum. The first instrument showed that something far more accurate than the thermopile of the period had been put

³⁴The Bolometer, by S. P. Langley, 'American Journal of Science,' Vol. 21, p. 187, 1881; Vol. 31, p. 1, 1886; Vol. 5, 4th Series, p. 241, 1898; Vol. 32, p. 90, 1886.

at the disposal of the physicist. The instrument has since been developed by Abbot³⁵ into a tool of extraordinary sensitivity and accuracy. The working part of the instrument consists of a thin strip of platinum P (see fig. 18), about 12 mm. long, 0.06 mm. wide, and its thickness may best be estimated from the fact that its electric resistance is about 4 ohms. For the sake of symmetry of conditions a second strip of platinum (not shown in the figure), as nearly as possible like the absorbing strip, in all respects, and mounted as close to it as practicable, but shielded from the radiation by a

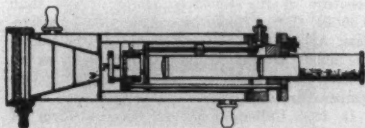


FIG. 18.—Diagram of Bolometer (Abbott's).

diaphragm D, forms the second resistance arm of the Wheatstone bridge. The two platinum strips are blackened by camphor smoke. The third and fourth arms of the bridge consist of two equal coils.

Special precautions are necessary for the avoidance of uncertain contacts, thermal effects, and for the protection from air currents, etc., but these are fully discussed in the papers mentioned. The figure shows diagrammatically the construction of the standard instrument at Mount Wilson Observatory. Experience has shown that the water cooling arrangement is

³⁵The Annals of the Astrophysical Observatory, Vols. I. and II. On page 28 and the following pages of Vol. II, a detailed description of the standard bolometer used at Mount Wilson will be found.

not necessary. An automatic photographic arrangement has been constructed which records the movements of the mirror attached to the galvanometer coil. In this way it has been found possible to obtain a bolograph, or in other words a photographic record, of the heat intensity of the whole solar spectrum from the extreme ultra-violet end at about 0.3μ to a wave length of about 3μ in the infra-red with ease and accuracy in about eight minutes of time. As giving some idea of the sensitiveness of the bolometer, it may be mentioned that Langley stated in 1898 that the instrument was capable of recording a change in temperature of the bolometer strips of less than 0.00001°C .³⁶ Notwithstanding its sensitiveness, Abbot is able to boast that the bolometer "is about as easy to use for this work as an ordinary thermometer is for measuring the temperature of the air."³⁷

In 1898 Callendar showed his first form of recording bolometer.³⁸ This was essentially a differential platinum thermometer, consisting of two equal flat resistances, one of which was blackened and the other left bright. These were mounted side by side, and were protected by a glass cover. The difference of resistance between them when exposed to sunshine was proportional to their difference in temperature and, owing to the greater absorption of heat by the blackened resistance, this was proportional to the intensity of the vertical component of the radiation. The difference in resistance was

³⁶The Bolometer, S. P. Langley, American Journal of Science, 4 Series, Vol. V., p. 242.

³⁷Address delivered before the Philosophical Society of Washington, Jan. 3rd, 1914. See "Science," Vol. XXXIX., p. 344.

³⁸A Quantitative Bolometric Sunshine Recorder, H. L. Callendar. Report of the British Association for the Advancement of Science, 1898, p. 796.

recorded on the now well-known Callendar Recorder³⁹ (fig. 19). It consists of a Wheatstone bridge, in which the movement of a slider along a bridge-wire is automatically effected by sensitive relays worked by the current passing through the galvanometer between the bridge

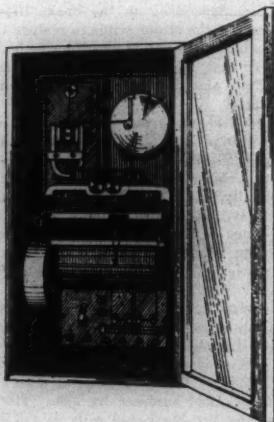


FIG. 19.—Callendar Electric Recorder.

arms. The moving coil of this galvanometer carries an arm which makes contact on one side or the other of a small platinum-rimmed wheel rotated by clockwork. When contact is made, a relay circuit is connected through one or other

³⁹Platinum Recording Pyrometer, 'Engineering,' May 26th, p. 675, 1899.

of two electro-magnets. Each of these magnets is mounted on a clock, the movement of which is prevented by a brake controlled by the magnet. When the current energises a magnet, this brake is lifted, allowing the clockwork to revolve. The clocks drive a differential gear and cord attachment by means of which the slider on the bridge-wire is pulled in one direction or the other, depending on the brake lifted. The movement of the slider tends to restore the electrical balance. A pen is rigidly attached to the slider so that all its movements are recorded on a chart placed round a clock-driven rotating drum.

Some difficulty was experienced in excluding damp from the original sunshine receivers, and Callendar in 1905 described⁴⁰ and exhibited his improved form of receiver in the hermetically-sealed glass bulb (see fig. 20). Unfortunately, with the exception of the note in the Proceedings of the Royal Society, in which some interesting bolometric records are reproduced, a description of this instrument was never published.

It consists of two pairs of resistance coils wound on mica frames, the two pairs being mounted diagonally to each other. One pair of coils is blackened by means of a black glass enamel, thus avoiding any risk of deterioration of the blackened surfaces by age. The resistance of the two blackened coils in the dark is the same as that of the two unblackened ones. The coils are placed in an hermetically-sealed glass vessel filled with dry air. The receiver is connected by means of four wires to the Callen-

⁴⁰Reports on the Total Solar Eclipse of Aug. 30th, 1905, Pt. I., by H. L. Callendar and A. Fowler. The Horizontal Bolometer, Proc. Roy. Soc. Series A. Vol. 77, p. 15, 1906. and see also the Radio-balance, by H. L. Callendar. Proc. Physical Soc. of London, Vol. XXIII. Dec., 1910, p. 4.

dar Recorder, which may be placed at a distance of many yards from the receiver. The instrument is standardised by comparing the readings obtained with those given by a similar instrument which has been standardised by measurements made on the radiation emitted by standard electric lamps. The lamps used by Callendar for this purpose emit a radia-

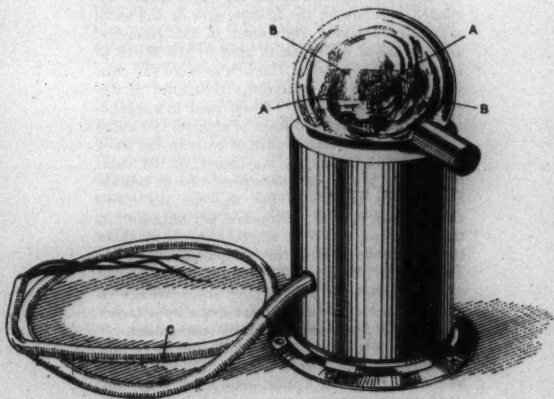


FIG 20.—Callendar Sunshine Receiver.

tion of approximately 1 calorie per square centimeter per minute at a distance of 20 cm. when a voltage of about 80 volts is applied to them.⁴¹ For steadiness of running this current

⁴¹See Report of the Committee on Solar Radiation, Report of the British Association, 1900, p. 38, and also the Radio-balance, *Ibid.*, p. 22.

is supplied by an accumulator battery. These lamps have now been in use for some years, and the results given by them have been found remarkably constant.⁴² Great care has to be taken to maintain the voltage on the lamp constant during an experiment as a variation of 1 per cent. in the voltage produces a variation of nearly 3 per cent. in the radiation. The scale of sensitiveness on which the records are recorded depends on the bridge wire or the automatically variable resistance in the recorder. In the case of instruments used by the Meteorological Office, London, the bridge-wire (20 cms. in length) has an equivalent resistance of approximately 0.7 ohms. When used in conjunction with the standard form of receiver, in which the resistance of either pair of coils in the dark is about 11 ohms, a movement of 10 mm. on the bridge wires corresponds to a definite change, 10 milliwatts per sq. cm., or 0.143 calories per square centimetre per minute, in the radiation received, whether the amount of the radiation is small or comparatively large. The actual intensity of the radiation received can be found by multiplying the readings in scale divisions on the recorder drum by a factor determined from the standard instrument.

To obtain the total number of calories in any given time the curve should be integrated by means of a planimeter. If so desired a planimeter may be attached to the recorder and the integration made automatically, the reading obtained being divided by the number of minutes in the time interval so considered. The integral gives the total energy received on a horizontal surface during the day; the quotient gives the average

⁴²In this connection it may be mentioned that the Bureau of Standards, Washington, is now issuing incandescent lamps as standards of radiation with certificates at a cost of about \$2.50 each.

intensity. Fig. 21 is the copy of a record obtained at the Meteorological Office, London, with one of these instruments, and is reproduced by the kind permission of the Director, the data on the sheet showing the method of reduction of the results.

An inevitable defect of any form of recording instrument which has been exposed continuously in all weathers without attention, is the necessity of employing a glass protecting envelope, which necessarily possesses some selective absorption. Fortunately, in the case of sunshine, the rays absorbed are a small proportion of the total radiation, but the method would not be satisfactory for a source at a low temperature. The error tends to a great extent to compensate itself, because the heated envelope radiates to the enclosed coil in nearly the same proportion as it absorbs; it is important, however, to employ very thin and uniform bulbs of low absorptive power.

A considerable amount of experimental work has been devoted to a study of the effect of the absorption by the envelope. The result of Marvin's work⁴³ tends to show that, if anything, the Callendar instrument is more sensitive to the violet and ultra-violet radiation than the other instruments in general use.

In comparing the results obtained with the Callendar instruments there is no doubt that each instrument has an absorbing factor of its own which must be taken into consideration. In his interesting comparison between the Angström and Callendar instruments, Patterson showed that if the Angström was covered with a piece of the same kind of glass as that used in the construction of the Callendar, "the intensities of the radiations given by the Ang-

⁴³The total radiation received on a horizontal surface from the sun and sky at Mount Weather Va. C. F. Murvin, 'Monthly Weather Review,' Vol. 42, No. 8, p. 474.

ström and Callendar instruments would be in very good agreement, with the possible exception of the weak radiation received during the early morning and late afternoon."⁴⁴

As showing the value of a bolometric recorder as compared with the Campbell-Stokes instrument, reference may be made to a paper by R. Corless⁴⁵, in which some record sheets obtained by a Callendar bolometer and recorder are reproduced. In the third figure considerable variations in the amount of radiation received are recorded, and the following remarks are made: "The curve is interesting from the fact that the record of bright sunshine for South Kensington on that day, obtained from a Campbell Stokes sunshine recorder exposed near the radiation receiver, was nil. The quantity of radiation received, as obtained from the area of the curve is, however, 64 calories per sq. cm., and the mean rate of radiation between sunrise and sunset is .007 watts per sq. cm."

The bolometer designed by Kurlbaum⁴⁶ is an extremely ingenious one, but, as far as is known, the instrument is not in general use for solar radiation measurements. Four similar bridge arms are made of platinum foil composed

⁴⁴See last paragraph of the Appendix in "Comparison of the Angström Pyrheliometer and Callendar Sunshine Recorder," by J. Patterson, Department of Marine and Fisheries, Meteorological Service of Canada, M.S. 50.

⁴⁵On the radiation records obtained in 1911 at South Kensington, together with a comparison between them and the corresponding absolute observations of radiation made at Kew Observatory, by R. Corless, Geophysical Memoir, No. 4, p. 59, Meteorological Office. M.O., No. 210d, 1912.

⁴⁶The Radio-Balance, H. L. Callendar. Ibid., p. 9. and Bolometrische Untersuchungen, von Dr. O. Lummer und Dr. F. Kurlbaum, Annalen der Physik und Chemie Vol. 46, p. 204, 1892.

of 12 connected strips 32 by 1 mm. and .001 mm. thick, the two diagonal arms being placed one behind the other and exposed to the radiation, the strips of the second being behind the spaces of the first. The bolometer is balanced against manganin resistances, which are not appreciably affected by the current, and it is therefore impossible, during the absolute measurement, to compensate for changes in the surrounding temperature in the usual way by balancing the bolometer exposed to radiation against a precisely similar grid. Owing to the high sensitiveness of the bridge, it is necessary that the temperature of the bolometer should not change by 0.0001° C., and this prevents its employment, except in laboratories where the temperature can be carefully controlled.

Like Kurlbaum's pyrheliometer, the bright and black bolometer of Callendar,⁴⁷ as distinct from the instrument previously described, is extremely sensitive, but difficult to make, and too delicate for general use. Although useful for the measurement of low temperature radiation, it need hardly be considered as a pyrheliometer.

3. The Angström compensated pyrheliometer⁴⁸ is perhaps the best known of the modern instruments for the measurement of solar radiation. This is partly due to the fact that it was temporarily adopted by the International Meteorological Congress at Innsbruck, 1905,⁴⁹ and also by the International Union for Co-

⁴⁷The Radio-balance, H. L. Callendar, *Ibid.*, p. 9.

⁴⁸Pyrheliometer, by Knut Angström, *Astro-physical Journal*, Vol. IX., 1899, pages 332-346, and *Actes de la Société royale des Sciences d'Upsal*, 1886.

⁴⁹Report of the International Meteorological Conference at Innsbruck. M.O. 195, p. 19, Meteorological Office, London.

operation in Solar Research, Oxford, 1905,⁵⁰ as the standard pyrheliometer.

The three first clauses in the memorandum presented by M. Violle to the Meteorological Congress are of some interest historically, and are as follows:—

(1) It is highly important to organise regular measurements of the total solar radiation at all meteorological observatories. These measurements should include hourly observations with direct reading instruments as well as continuous records with autographic instruments.

(2) Angström's compensation-pyrheliometer should be used for the eye-observations. It has been recognised as the most suitable instrument for this purpose.

(3) Various arrangements may be used for autographic records. The selected actinometer may be adopted to record mechanically, electrically, or photographically. When deciding on a form of instrument, accuracy and sensitiveness must be taken into account, as with all good measuring instruments. It goes without saying that the instrument must be calibrated, and that it must be verified from time to time by comparing its indications with those of an absolute standard."

It is regretfully to be noticed that, as far as the author is aware, no further progress in the development of autographic pyrheliometers has been made since 1905.

The resolutions adopted at the International Union at Oxford were as follows:—

(1.) In order to secure uniformity, it is desirable that observations on the intensity of solar radiation in different localities shall be made as far as possible with the same type of instrument.

⁵⁰The compensating pyrheliometer. Transactions of the International Union for Co-operation in Solar Research, Vol. I., p. 178-180. Manchester.

(2.) That for the present Angström's pyrheliometer be adopted as the standard instrument.

(3.) That it is desirable to obtain accurate comparisons between the records of Angström's pyrheliometer and other standard instruments.

The principle of the Angström pyrheliometer is simple. Two thin metal strips⁵¹ (20 mm. long, 1.5 mm. wide, and .02 mm. thick) are alternately exposed to the radiation to be measured. When one strip is being heated by the radiation falling upon it, the other strip, which although shielded, is close to the exposed strip, is heated to the same temperature by an electric current passing through it. Equality of temperature is indicated by a sensitive galvanometer connected to a pair of copper-constantan thermo-junctions, attached to the back of the receiving strips, but insulated from them by thin silk paper and shellac varnish.

It is then assumed that the energy expended in the electrically-heated strip is equal to the radiant energy absorbed by the exposed strip.

The amount of energy absorbed by each particular instrument depends on three factors, which must be accurately known, viz., the width and resistance of the strips and the coefficient of absorption of the smoke-black film on the strips.⁵²

Knowing these values, the absolute value of the radiation may be determined.

⁵¹As originally constructed by Angström, the strips were of platinum, but Callendar, having shown that the temperature co-efficient of this metal was a serious source of error, strips of manganin are now employed.

⁵²The absorption co-efficient is generally given as 98 per cent. in a new instrument—the evidence seems to show that the black deteriorates in time, the absorption co-efficient becoming smaller.

The radiated energy absorbed by unit length of the strips = $q a b$, where

q = the radiation in C.G.S. units,

a = the co-efficient of absorption, 0.98,

b = the width of the strip in centimetres.

Again, if i be the intensity of the current and r the resistance of unit length, then the electrical

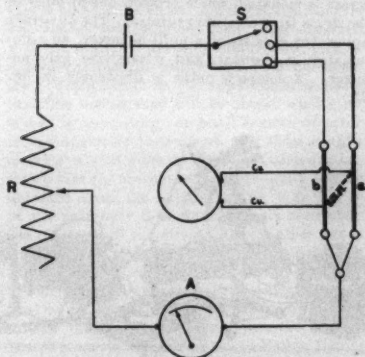


FIG. 22.—Diagram of the Electrical Connections of the Angström Pyrheliometer.

power used to balance the radiated energy is ri^2 watts.

$$\text{Accordingly } q a b = ri^2 \text{ and } q = \frac{ri^2}{ab}$$

This formula gives the strength of the radiation in watts per sq. cm.

The observations published in this country in the 'Geophysical Journal' are expressed in these units. The radiation in calories per minute can be found by multiplying by the factor

$$\frac{60}{4.18} \times$$

The construction of the instrument is simple, and is illustrated diagrammatically in fig. 22, the electrical connections being also shown. The two strips of manganin, *a* and *b*, are mounted side by side on a small circular frame about 40 mm. in diameter. On the front end of the tube carrying the strips a double-walled reversible screen is mounted, which protects one or other of the strips from external radiation. The auxiliary apparatus consists of a milli-ammeter, an accumulator, a rheostat, and a sensitive galvanometer. A complete outfit is illustrated in fig. 23.

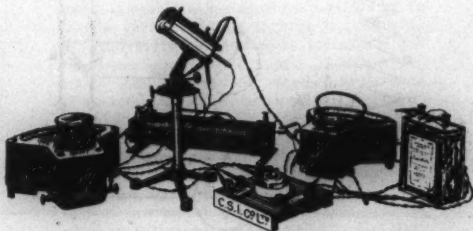


FIG. 23.—Angström Pyrheliometer with Accessories.

W. Marten has published⁵³ a comparison between five Angström pyrheliometers. He showed that they agreed within 2 per cent. of each other, but gave results ranging from 3.5 to 5.3 per cent. lower than the standard water flow pyrheliometer using the Abbott silver-disc pyrheliometer as a subsidiary standard for the test. In the hands of an experienced observer the

⁵³Vergleichsmessungen mit Pyrheliometern, von W. Marten, Ergebnisse der meteorologischen Beobachtungen in Potsdam, 1911, p. 11.

Angström is found to be a reliable instrument, but it is imperative that new instruments should be thoroughly tested before observations are commenced. After five years' use no change in the constants was found in any of the five instruments.

Recent work by Abbott⁵⁴ also shows that the values obtained with the Angström instruments are almost always lower than those obtained with his standard pyrheliometer. The mean of a large number of values shows 3.9 per cent. low. Experiments as yet unpublished indicate that this discrepancy will be almost wholly removed by correcting two small sources of error in the Angström instrument. To those sources of error Abbott gives no clue, but Callendar has shown that the thermo-couples gradually become detached from the heating strips, and that it is also extremely difficult to measure accurately the constants of the strips. On the other hand, Paschen⁵⁵ has shown that these errors are largely due to lack of uniformity in the thickness of the strips. It must also be borne in mind that the two conditions of heating the strips are not exactly the same. In the one case the strip is heated from the exterior by the radiation falling upon it, in the other from the interior by the current passing through it. The radiating conditions of the strips are, therefore, not exactly similar.

4. Tyndall improved the Pouillet pyrheliometer by substituting mercury for water in the box which absorbs the radiation, but with that exception little modern work had been done with this type of pyrheliometer until the authorities of the Smithsonian Astrophysical Observatory brought out their mercury pyrheliometer in

⁵⁴Annals of the Astrophysical Observatory of the Smithsonian Institution, Vol. III., p. 166.

⁵⁵Ueber die absolute Messung einer Strahlung von F. Paschen, Annalen der Physik, Vol. 38, pp. 30-42, 1912.

October, 1902.⁵⁶ This instrument was superseded in turn by the silver-disc pyrheliometer.⁵⁷

This pyrheliometer has now been largely adopted in the United States as a subsidiary standard, about twenty-five copies having been standardised and sent out to stations in Europe, North and South America, and other places.

A section of a silver-disc pyrheliometer is shown in fig. 24. The silver disc *a* is bored radially with a hole to admit the cylindrical bulb of a thermometer *b*. The hole in the disc has a thin lining of steel, so that a small quantity of mercury may be introduced without alloying with the silver, in order to make a good heat conduction between the silver disc

⁵⁶Mention must be made of the work of the Committee appointed by the British Association for the Advancement of Science for the purpose of considering the best methods of recording the direct intensity of solar radiation. In the third and fourth reports of the Committee, 1886 and 1887, a description is given of the actinometer made for the Committee. It consisted of a copper cube of about 90 mm. square outside, in the interior of which was a thermometer, on the bulb of which the radiation was allowed to fall through an aperture. In a later modification of the instrument a metal disc in which a thermo-couple was mounted was substituted for the mercury thermometer, but no results of any value appear to have been obtained with the instrument. In this connection Crova's registering actinograph should be mentioned. A thermo-couple in the form of a disc received the solar radiation, and the rise in temperature of the disc was recorded on a photographically recording galvanometer. See "Report on Radiation," by J. Violle. Ibid. 52.

⁵⁷Annals of the Astrophysical Observatory of the Smithsonian Institution, Vol. II., p. 36, Vol. III., p. 47, and Smithsonian Miscellaneous Collections, Vol. 56, No. 19.

and the thermometer bulb. A soft cold soaked in shellac is forced down at the mouth of the hole to prevent the escape of mercury. The thermometer *b* is bent at a right angle and is protected by a brass tube; it is graduated in tenths of a degree Centigrade from -15° C. to $+50^{\circ}$ C. Before insertion in the instrument a careful calibration of the stem is made. The silver disc is enclosed by a copper cylindrical box *c*. Three small steel wires not shown in the figure, support the silver disc. Clamping screws

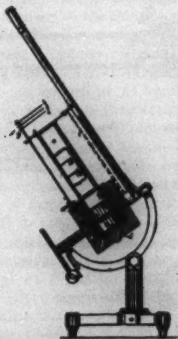


FIG. 24.—Silver-disc Pyrheliometer.

are provided in order that all strains may be taken off the wires during transit. The box *c* is enclosed in a wooden box *d* to protect the instrument from temperature changes. Sunlight is admitted through the tube *e*, in which a number of diaphragms are placed. The aperture in the diaphragm *f*, nearest the silver disc is slightly smaller than the others and slightly smaller than the disc itself. It thus limits the cross section of the beam whose intensity is to be

measured. Shutters are provided at *h*. The top of the tube *c* carries a screen *k* large enough to shade the wooden box *d*. The instrument is mounted on an equatorial stand. When taking an observation the instrument is pointed at the sun and the temperature rise of the thermometer during intervals of 100 seconds is observed over a few minutes. From the readings obtained the radiation can be deduced.

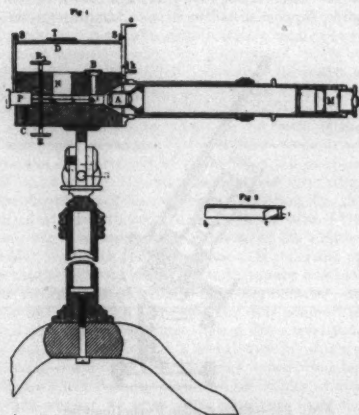


FIG. 25.—Michelson's Pyrheliometer.

It must not be forgotten that the silver-disc pyrheliometers only furnish readings proportional to the intensity of radiation of the sun, and are comparable one with another, but they do not provide independent means of reducing these readings to true heat units. The instruments have the outstanding advantage of simplicity, and at the same time accuracy. It is

found that the standard scale of radiation may be reproduced in these instruments to at least an accuracy of 0.5 per cent.⁵⁸

The Michelson pyrheliometer⁵⁹ (see fig. 25) is similar to the Smithsonian instrument in that the heat rays are concentrated directly on to a thermometer. In this case the thermometer consists of a bimetallic strip, *a*, *b*, the extremity of which, *i*, moves when the strip is heated by the solar radiation, admitted through the aperture *D*. The movement of the strip is observed by means of a microscope, *M.A.* The instrument is calibrated, and an observation is made in a similar manner to the Smithsonian instrument, but the sensitive element having a smaller thermal capacity, the instrument is more rapid in action. It is doubtful, however, whether it is so robust or so constant in its readings as the American instrument.⁶⁰

In this same group of pyrheliometers the black bulb thermometer *in vacuo* should be included although it is not now seriously considered as an instrument for measuring the solar intensity. Nevertheless, a large number of these instruments are still in use, although it is difficult to satisfactorily interpret the results obtained with them. The construction of the instrument was first suggested by Sir John Herschel. It consists of a sensitive maximum thermometer (see

⁵⁸The Solar Constant of Radiation, C. G. Abbott, "Science," Vol. xxxix., p. 345.

⁵⁹Ein neues Aktinometer, by W. A. Michelson, St. Petersburg; Meteorologische Zeitschrift, 1908, p. 246.

⁶⁰Experience has shown that in the Fery spiral radiation pyrometer (see "Engineering," 87, p. 663, 1909), which in principle is an almost similar instrument, these small spirals are remarkably constant in their behaviour, although they may have been subjected to comparatively high temperatures for long periods of time.

fig. 26), having the bulb and about 25 mm. of the stem coated with dull lampblack. The whole is then enclosed in a glass tube, of which one end is blown out into a bulb about 55 mm. in diameter, in the centre of which the bulb of the thermometer is fixed. The glass jacket is then exhausted. Great difficulty has always been experienced in finding any two black bulb thermometers which will be thoroughly consistent in their indications. G. M. Whipple⁶¹ showed that the size of the bulb had a very large effect on the readings and consistency of the thermometers, a difference of 1 mm. in a bulb of 12.5 mm. in diameter, or a difference of 8 per cent. causing the correction of the smaller bulb thermometer—as compared with the standard instrument of the Observatory—to be more than doubled. He also found that the thickness of blacking had some considerable influence on the readings obtained. The experiments indicated “the necessity of exacting from instrument makers a rigid adherence to a standard size of bulb, and a definite amount of coating, as well as a constant state of exhaustion and standard size of jacket.”

An earlier paper⁶² showed that at Kew Observatory there was a very intimate association between the intensity of radiation as indicated by the black bulb thermometer and the number of hours of sunshine. In a discussion of graphs showing daily observations for a year he found

⁶¹Preliminary enquiry into the causes of the variations in the readings of black-bulb thermometers *in vacuo*, by G. M. Whipple, Quarterly Journ. of the Meteorological Society, Vol. X., p. 45, Jan., 1884.

⁶²On the relation existing between the duration of sunshine, the amount of solar radiation and the temperature indicated by the black-bulb thermometer *in vacuo*, by G. M. Whipple. Quarterly Journal of the Meteorological Society, Vol. V., p. 142, 1879.

that, taking both maxima and minima into consideration, there were seventy cases of agreement between the solar radiation and sunshine in their times of occurrence, and six of disagreement, whilst the former phenomena preceded the latter by a short interval ten times, and succeeded it sixteen times.

He was led to the conclusion that "the black-bulb thermometer is only to be considered as an indication of the relative presence or absence of cloud from the sky at the locality, and so its use as a meteorological instrument may with advantage be set aside in favour of the sunshine recorder, which is not influenced by the element of uncertainty inseparable from the former instrument." At the present time many observers continue to com-



FIG. 26.—Black-Bulb Thermometer in Vacuo.

pare blacked and unblackened thermometers *in vacuo*. No doubt the difference in the readings of the two thermometers gives some idea of the intensity of the total radiation.

Winstanley's radiograph⁶³ was very similar in principle to the Dines recording sunshine receiver, except that it was intended to record the intensity of solar radiation, and not only

⁶³Winstanley's Recording Radiograph—"Engineering," Oct. 15th, 1880, and Air Thermometers, D. Winstanley, Phil. Mag., Nov. 1880, p. 380. Winstanley's invention was, however, anticipated by the Abbé Allégret, who, in 1870, mounted a differential thermometer on an axis so as to form a sort of balance, which he called the "solar counter." See J. Violle, Report on Radiation. Ibid., p. 45.

duration of sunshine. A differential black-bulb and unblackened-bulb thermometer (shown diagrammatically in fig. 27) was poised upon knife-edges so delicately adjusted that its equilibrium was disturbed by the displacement of the mercurial index, equilibrium being re-established by the whole system taking up a fresh position. The movement from one position of equilibrium towards another was recorded by a long arm upon a sheet of smoked paper wrapped round a drum driven by clockwork, the movements being proportional to the intensity of radiation received. In practice the instrument

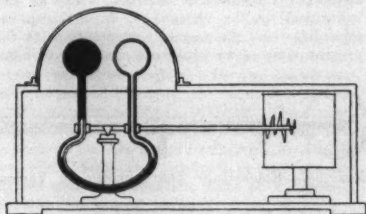


FIG. 27.—Winstanley Radiograph.

was found to be very sensitive, and it is to be regretted that further experiments were not made to evolve a satisfactory meteorological instrument.

J. Richard⁶⁴ has introduced a recording actinometer of the Violle type, in which his well-known metallic thermometers are employed. The bulb of the thermometers, one of which is unblackened, and the other bright, are protected by glass spheres. The bulbs of the thermometers are connected by capillary tubing to recording mechanism of the Bourdon gauge type. The

⁶⁴See Catalogue of Meteorological Instruments, Jules Richard, Paris.

two thermometers record on the same drum, so that the difference of their readings and the times of their respective maxima can be observed.

A simple and ingenious instrument was designed by the late W. E. Wilson⁶⁵ for the measurement of total radiation received by the ground. The radio-integrator (see fig. 28), as it is named, consists of a sealed retort for the distillation of a volatile liquid, in vacuo, by the radiation falling upon it. When set for an

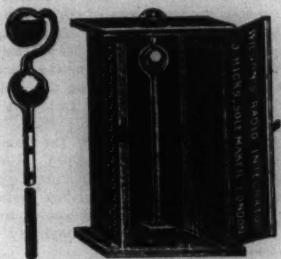


FIG. 28.—Wilson Integrator.

observation, the whole of the liquid is in the upper bulb, which is exposed to the sun while the lower limb and tube are sheltered in a white perforated box. The liquid as it evaporates is condensed in the lower bulb and trickles down into the tube. As the latent heat of vaporisation of alcohol is constant at all ordinary air temperatures, the amount of alcohol which distills over from the upper bulb is directly proportional to the solar radiation. As generally constructed, a cubic centimeter of alcohol passes

⁶⁵See catalogue published by J. J. Hicks, Hatton Garden, London.

over for 179 gram calories received by the upper bulb. Unfortunately, the instrument is sensitive to the cooling effect of the wind. This error might perhaps be corrected by enclosing the upper chamber of the instrument in a vacuum jacket.

5. In 1903 Abbott and Fowle⁶⁶ commenced the construction of their absolute pyrheliometer composed of a black-body receiver combined with a flow calorimeter, the chief innovation being the adoption of a hollow absorbing chamber to receive the solar rays. Such a chamber has approximately the properties of the "absolutely black-body," and is almost a perfect absorber.⁶⁷ Consequently no correction is needed for the reflection of rays from the receiving surface. Abbott states that when designing this "black-body" he was unaware of the fact that this principle had been employed in pyrheliometry by V. A. Michelson, since 1894.⁶⁸ The standard pyrheliometer has been developed through various models into a wonderfully accurate piece of apparatus. The standard water-flow pyrheliometer, No. 3 (see fig. 29) must appeal to all lovers of instruments by the simplicity and effectiveness of its design.⁶⁹

⁶⁶Annals of the Astrophysical Observatory of the Smithsonian Institution, Vol. II., p. 40.

⁶⁷Some interesting photographs of the appearance of three receiving cones, equally blacked, of which the angles are respectively 60°, 45°, and 30°, show clearly how much more efficiently the deeper cone absorbs the radiation than the shallower ones. See "Les lois du rayonnement calorifique," by C. Fery, Bulletin de la Société d'Encouragement pour l'Industrie, Dec., 1909, p. 7.

⁶⁸See V. A. Michelson, Russkoe fiziko-kimicheskoe obschestvo zhurnal, St. Petersburg, 1894. Vol. XXVI. Pt. 1, 1-25.

⁶⁹The Annals of the Astrophysical Observatory, Vol. III., pp. 52-72.

AA is a chamber of about 3.5 cm. inside diameter, whose walls are hollow, and adapted for the circulation of a stream of water. The stream enters at E and bathes the walls and rear of the chamber and the cone-shaped receiver of rays H, and passes off at F, carrying away the heat developed by the solar rays which entered the chamber by the measured orifice C. At D_1 and D_2 are platinum coils adapted to measure the rise of temperature of the water due to the heating. The vestibule BB through which the rays pass is bathed by the outflowing water which escapes at F. The water flows in a spiral channel round and round the vestibule B, so that it is effectually kept at a constant temperature. The circulation of the inflowing water

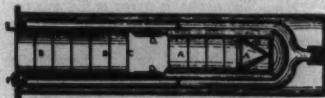


FIG. 29.—Water-flow Pyrheliometer.

in the walls of the chamber AA is also effected by means of a spiral channel cut from the front to the back, thence by a spiral of decreasing radius down to the centre of the back of the chamber, then by a spiral on the cone-shaped piece H until it emerges into the tube which leads to D_2 . In order to prevent outside temperature influences from vitiating the observation, the whole apparatus is enclosed in the Dewar vacuum flask KK, and to prevent the breakage of the flask it is enclosed in a brass tubular receptacle.

The standard instrument No. 4 is somewhat similar, but the Dewar flask has been discarded in favour of a vigorous water stirring arrangement. Reference should be made to the original paper for information as to the construction of

the resistance thermometer coils, the method of maintaining the water flow constant, etc. On the back of the receiving-cone H, a coil of manganin wire is wound, forming a heating coil by means of which a definite quantity of electrical energy can be dissipated in the instrument. In this way a direct comparison may be made between the temperature rise in the outflowing water caused by solar radiation and that caused by a known quantity of electrical energy.

The published observations show that the results obtained with these standard instruments are remarkably concordant, the maximum divergence of the mean results of six groups derived from seventy observations being 1 per cent. It is believed that the mean result of all the comparisons must be within 0.5 per cent. of the truth. The probable error is 0.1 per cent.⁷⁰

Mention should be made of an instrument designed by Féry, in which he forms a receiving cone inside the bulb of an air thermometer.⁷¹ This cone is wound with a heating coil like the Smithsonian pyrheliometer, and can be electrically standardised in a similar manner. It is improbable that the instrument is capable of the accurate work of the Smithsonian instrument.

6. The radio-balance due to Callendar was developed from the radio-calorimeter described by him in the British Association Report for 1900.⁷² In this instrument a copper disc (13 mm. in diameter) was supported by two fine iron-constantan couples at right angles, either of which could be used for observing the rate of rise of temperature of the disc, when exposed

⁷⁰Annals of the Astrophysical Observatory, Vol. III., p. 72.

⁷¹Sur la constante du rayonnement, C. Féry and M. Drecq. Comptes Rendus, March 6th, 1910.

⁷²Report of the Committee on Solar Radiation, British Association, 1900, p. 37.

to radiation. It occurred to Callendar⁷³ that by passing an electric current through one of the thermo-couples, the radiation might be directly compensated by the heat absorption due to the peltier effect. In the first model a disc was used as a receiver, but in the latter and more successful model a cup (4 mm. diam., 10 mm. deep) is employed. This cup has been found experimentally to have an absorption coefficient of 100 per cent.⁷⁴

The radiation is directly compensated by the heat absorption due to the peltier effect in a thermo-junction, soldered to the cup, through which a measured current is passed, and is deduced in absolute measure without a knowledge of the thermal capacity of the cup. The cup is mounted in a small tubular thermopile connected to a resistance galvanometer for indicating the balance.

In order to compensate accurately for changes in the surrounding temperature and to avoid the necessity for a water jacket, two similar cups with similar connections are mounted side by side in a hollow copper cylinder. Either can be connected by a double pole switch to a circuit of the battery B, rheostat R and milliammeter A for supplying, regulating, and measuring the current (see fig. 30). The piles in which the cups are mounted are oppositely connected in the circuit of a sensitive galvanometer G, so as to indicate the difference in temperature of the cups. The upper junctions of each

⁷³The Radio-Balance, H. L. Callendar, Proc. Physical Soc., of London, Vol. XXIII., p. 10.

⁷⁴It was found that, if the interior of the cup was not blackened, but left as it came from the spinning, its effective co-efficient of absorption was 94 to 96 per cent., as against 30 per cent. for a flat disc in a similar condition. It was a fair inference that if the interior of the cup were blackened, its co-efficient of absorption would be practically equal to 100 per cent.

pile are bound firmly round the middle of a cup from which they are insulated by thin paper and shellac (see fig. 31). The lower junctions are similarly fastened to a small copper block screwed to the base of the thick copper cylinder. The piles usually consist of twelve couples each, giving seven times the sensitiveness of a single couple. The peltier couples are single iron-constantan couples soldered to the bottom of the cups. The four pairs of leads for the

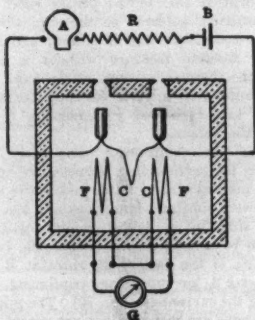


FIG. 30.—Diagram of Wiring of Callendar's Radio-balance.

two couples and the two piles are brought out separately, so that the insulation can readily be tested at any time.

The equation giving the value H of the radiation in absolute measure (watts per sq. cm.) is

$$aAH = PC - C^2R = PC(1 - C/C_0),$$

where a is the absorption co-efficient of the blackened cup,

A the area of the aperture admitting radiation in sq. cms.

H the intensity of the radiation in watts per sq cm.

P is the co-efficient of the peltier effect in volts.

C the current through the couple in amperes.

$C_0 = P/R$, the neutral current giving neither heating nor cooling.

It is obvious that there is a maximum radiation which can be directly compensated by the cooling effect, but the range can be varied, and is quite suitable for the measurement of solar radiation.

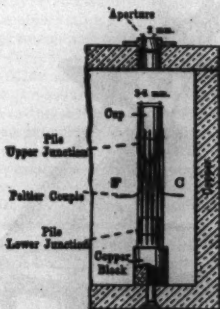


FIG. 31.—Part Section of Callendar's Radio-balance. Cup form.

For data as to the value of the peltier co-efficient, the values of the neutral currents, methods of standardising, etc., reference should be made to the original paper.

When the radio-balance is employed as a pyrheliometer the thick copper cylinder containing the cups and their connections is extended by a tube containing suitable diaphragms for limiting the exposure. The whole is mounted equatorially on a levelling stand, as shown in fig.

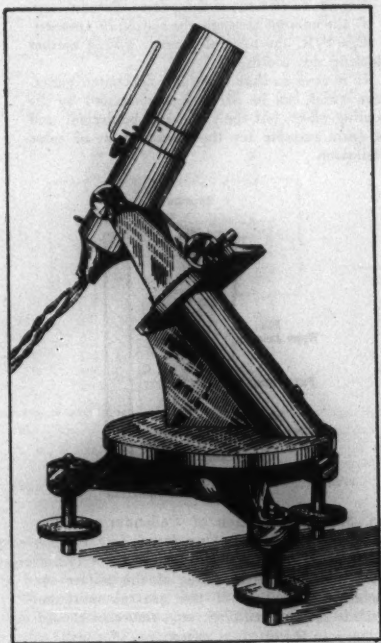


FIG. 32.—Radio-balance in Telescope.

32. The tube is gilt in place of being lacquered, as the gilt stands exposure to sunshine better, and also greatly diminishes the absorption of heat. There are no external terminals to the radio-balance, and this eliminates a possible source of trouble. An advantage of the direct compensation method is that the result is obtained by a single observation, but it is generally desirable to interchange the cups at each observation.

The chief advantage of the direct compensation method, especially when a variable like sunshine is being measured, is the rapidity with which an observation can be made ($1\frac{1}{2}$ minutes). The most convenient method of observation is to set the current at a suitable constant value, such as 0.200 ampere, corresponding to nearly 1 calorie per square centimetre per minute, and to observe the deflections of the galvanometer, which are proportional to the change in the intensity of radiation. The instrument is almost perfectly compensated for changes in the surrounding temperature, and it quickly reaches a final steady deflection. A disadvantage is that the increase of the peltier effect per 1°C . for the couples employed is nearly .0001 volt in 0.0150 volt. It is necessary therefore to read the temperature to 0.1°C . (by means of the thermometer shown in fig. 32), in order to obtain an accuracy of 1 in 1,500.

7. The radiometer of Crookes, although a very sensitive instrument, has not proved of service practically. The laboratory form of the apparatus consists of two blackened mica vanes suspended with a quartz fibre *in vacuo*. One side of each vane is blackened, the other left bright. Radiation falling on a vane deflects the suspended system, the movement being observed with a telescope and scale. The laboratory instrument is not sufficiently reliable for accurate work, and the pivoted vane type which we all enjoy watching in the shop window has not emerged from the toy form.

Conclusion.

From the foregoing it will be seen that the instruments for measuring solar radiation fall into three main groups, viz. :—

Those receiving direct radiation from the sun.

Those receiving radiation on a horizontal surface only, and

Those receiving radiation on a spherical surface.

These groups may in turn be sub-divided into those instruments which may be standardised directly in themselves, and those which are subsidiary standards being standardised on either other instruments, or on sources of heat emitting a known amount of radiation.

THOSE RECEIVING DIRECT RADIATION FROM THE SUN.

TYPE.	NAME OF INSTRUMENT.
Spectroscopic...	Bolometer, Paschen & Coblentz Thermo-piles.
Total Radiation	Water-flow,* Angström,* Radio- balance,* Michelson, Abbott silver-disc, Violle.
Duration ...	Campbell-Stokes, Jordan, Mar- vin, Dines.

The three instruments marked with an asterisk in the above table may be considered as absolute standards in that they may be standardised directly in themselves, and are thus independent of outside standards other than electrical accessories.

THOSE RECEIVING RADIATION ON A HORIZONTAL
SURFACE ONLY.

TYPE.	NAME OF INSTRUMENT
Recording ...	Callendar.

THOSE RECEIVING RADIATION ON A SPHERICAL
SURFACE.

TYPE.	NAME OF INSTRUMENT.
Maximum ...	Black-bulb.
Recording ...	Winstanley, Richard.
Totalising ...	Wilson.

Accurate comparisons have been made between the Smithsonian and Angström instruments and also between the Callendar radio-balance and the Angström, but as far as the author is aware no direct comparisons have yet been made between the radio-balance and the water-flow instruments.

The radio-balance and the water-flow instruments agree in showing that the values obtained from the Angström instrument are low (see page 42) as compared with the water-flow instrument; the Angström reads 3.9 per cent.⁷⁵ and, compared with the radio-balance, 1.6 per cent. low.⁷⁶ There is, therefore, apparently, a discrepancy of about 2 per cent. between the water-flow and

⁷⁵Annals of the Astrophysical Observatory. Ibid., Vol. III., p. 72.

⁷⁶The Radio-balance, Ibid., p. 25.

radio-balance instruments. The accuracy of the observations in both cases is of a high order and the concordance in the results obtained with each type of instrument remarkable. In the case of the water-flow instruments, the errors do not exceed 0.5 per cent., and are most probably not greater than 0.1 per cent.⁷⁷

The following instruments have come into general use,

(a) The Smithsonian silver-disc pyrheliometer.

(b) The Michelson pyrheliometer.

(c) The Callendar recording bolometer.

The silver-disc instrument is essentially a practical and robust one, and a long series of inter-comparisons between various instruments has shown that they are constant to within 0.5 per cent. and consistent. As has been previously mentioned, a number of these instruments have now been distributed throughout the world, and some interesting data will soon be available for discussion.

The Michelson instrument is largely used on the Continent, more especially in Russia, but the author has no information as to its constancy.

The Callendar recording bolometer has the outstanding advantage as compared with all other bolometers (with the exception of the elaborate recording water-flow instrument of the

⁷⁷Coblentz, who made a modified form of radio-balance, compared it against a Callendar sunshine recorder, which had been previously calibrated against the Abbott primary standard pyrheliometer. As a result of the comparison he states that the radio-balance gave values which were about 5 per cent. below the normal. Varying the position of the couples, etc., gave other values. The author feels that these tests are not conclusive, and further investigation is required.—“Instruments and Methods Used in Radiometry, by W. W. Coblentz: Bulletin of the Bureau of Standards,” Vol. ix. p. 51-55.

Smithsonian Institution) of being recording. Not only does it record the total intensity of vertical radiation during the twenty-four hours, but also the radiation at any particular time. If the receiver is mounted equatorially, it can be arranged to record the direct solar radiation whilst the sun is above the horizon. All the instruments that are distributed throughout the world have been previously standardised by Callendar, and have, as a whole, agreed well with his absolute standard. As previously mentioned (p. 34) some experiments are now being commenced with a view to determining whether the glass envelope surrounding the receiving coils can be reduced in thickness to minimise the radiation absorbed by the glass.

INSTRUMENTS OF TO-DAY.

In the case of the other instruments for measuring the intensity of radiation, the bolometer and thermopiles require an elaborate equipment, and can only be operated in an observatory. The black-bulb, Violle, and most of the older types of actinometer, have been superseded by the newer forms already described. The Wilson radio-integrator appears to have possibilities, but requires study and modification. The Campbell-Stokes still appears to be the most satisfactory instrument for recording the duration of sunshine, assuming that the instrument has been carefully adjusted when placed in position. The Marvin recorder is used largely in the United States of America.

Unfortunately the workers in solar radiation problems in the British Isles are much handicapped by climatic conditions. The results obtained in high altitudes⁷⁸ are more satisfactory

⁷⁸In this connection it is interesting to note that experiments are being made jointly by the Smithsonian Institution and the United States Weather Bureau from sounding balloons to determine the solar radiation constant. The meas-

than those obtained in lower ones, so that the British worker is handicapped not only by want of altitude, but by the fact that he is working in an atmosphere containing a quantity of moisture.

A considerable amount of evidence is collecting showing that the solar constant, or as Poynting prefers to call it, the "Solar Radiation Stream,"⁷⁹ is variable. From a comparison of the fluctuation in the number of sun spots it appears that the greater the number of sun spots, the stronger will be the Radiation Stream. This, as Abbott points out, is a very curious circumstance, as the temperature of the earth is generally lower at sun spot maximum than at sun spot minimum.⁸⁰

The mean value of the solar constant determined from 690 measurements made by the Smithsonian Astrophysical observers for the epoch 1902 to 1912 is 1.93 calories per square centimetre per minute (135 milli-watts per sq. cm.). This value shows that the radiation from a square centimetre of the sun's surface is about 6,000 watts, or roughly, 8 horse-power.

The author has gratefully to acknowledge the kind suggestions, and help he has received in the preparation of this paper, from Mr. T. H. Burnham, Prof. H. L. Callendar, Mr. R. H. Curtis, Mr. W. H. Dines, Mr. C. C. Mason, and especially his brother, Mr. F. J. W. Whipple. He has also to acknowledge with thanks the loan of clichés from The Cambridge Scientific Instrument Company, Messrs. C. F. Casella & Company, Mr. J. J. Hicks, The Royal Meteorological Society, The Physical Society of London, and Messrs. Negretti & Zambra.

urements unite in indicating values of the solar radiation at 10,000 metres which fall below the value of the constant as obtained by other methods. The experiments are, however, in their infancy.—"Science," Vol. xxxix. p. 348.

⁷⁹"Heat," Poynting and Thomson, p. 255.

⁸⁰"Science," Ibid., p. 347.

Discussion.

THE PRESIDENT said they had listened that night to a fairy tale of science. Apart from the intrinsic interest of the paper, it had been most fascinating to see how an apparently simple problem had been made difficult by those who were engaged upon its solution—(laughter)—and how they had attacked their own difficulties. But he could never see a sunshine recorder without thinking it had a romance of its own, because in 1789 the French Revolution had broken out, and there was a passage in history which spoke of the influence of the sun's rays there. He did not know whether Mr. Whipple was acquainted with the episode, but it might be interesting to recall it. They had in those days little scientific toys, and one of them was a brass cannon fired by the sun as it passed its meridian. One Sunday—he thought it was July 12th, 1798—there was a very excited crowd in the Palais Royale, Paris. News had come through that the King had refused to sanction the requisitions of the National Convention, then sitting at Versailles. And while this crowd of malcontents were considering what they should do under the circumstances, the sun passed its meridian and the cannon went off. A French oculist of the day, who was a most disreputable member of his (the President's) profession, named Jean Paul Marat—(laughter)—was editor of a paper called "Le Voix du Peuple," and in that paper it was recorded what a tremendous sensation was caused in that assembly by the solemn roar of that little gun, how Camille Desmoulins sprang on a table, seized the leaves from the boughs of a tree, and placed them in his hat as a kind of cockade, how all the crowd did likewise, and how, two days afterwards, the Bastille fell. Well, it seemed to him that that little gun, fired by the sun, was an instance of solar radiation having a marked influence on human progress. (Laughter.) It still reverberated, for the cannonade

which was now going on all over the world, practically against despotism, was still the outcome of the idea which then sprang into being. For there was no doubt that the French Revolution was the commencement of the true nineteenth century, and of the influences of humane thought which separated despotism from freedom. They had with them that evening a galaxy of stars of the first magnitude—he thought it better to keep his description as astronomical as possible. (Laughter.) They had Sir Frank Dyson, the Astronomer Royal, Dr. Chree, of the Kew Observatory, Mr. Marriott, of the Meteorological Society, and others who he hoped would join in the discussion of the scientific methods which had been brought to bear upon the solution of the problem of how much heat we were getting from the sun and what our "place in the sun" was. (Laughter.) This planet was 93,000,000 miles away from the sun—at least, so they were told—and it was only receiving a 2,000-millionth part of his beams, but nevertheless the problem was one of enormous importance. Looked at properly, it was of the greatest possible importance to human welfare. The recording of sunshine was valuable, too, from the point of view of the public health, because it was often forgotten that sunlight was one of the most powerful antiseptics in existence, and in modern medicine its value in the treatment of disease was being increasingly recognised.

SIR FRANK DYSON, Astronomer Royal, said it had given him very great pleasure to listen to Mr. Whipple's exposition of the development of the different methods of measuring the duration and the amount of solar radiation. It was, of course, a very long way from Campbell's original instrument to the wonderful modern devices of Callendar, Michelson, and Abbot. This history given by Mr. Whipple of Campbell's instrument had interested him very much. They had a Campbell instrument at the Greenwich

Observatory, but it had never occurred to him how many experiments had been made before that instrument arrived at its ultimate form, and it was most instructive to see how the original wooden bowl and glass bottle was replaced by a globe, and how many and various modifications had been made in such an apparently extremely simple apparatus as that glass ball. Certainly the apparatus as finished struck one as extremely beautiful in its simplicity. All the observations which had been described by Mr. Whipple were of interest in two different ways. First there was the economic aspect—the measuring both of the duration of sunshine and the amount of sunshine as it fell on any particular part of the globe, particularly in agricultural countries. And then, from the other side, there was the very interesting astronomical point of view, as to the amount and constancy of the sun's radiation. That question had been very carefully studied by Mr. Abbot by means of the very beautiful instrument which they had been shown. Mr. Abbot's observations were made in Algiers, while simultaneously observations were being made in America, and it had certainly been found that when there was a large ratio in Algiers there was also a large one in California; and Abbot had come to the conclusion that there was a real variation in the energy which the sun radiated—it might be as much as 10 per cent. He could not say he was altogether convinced by Abbot's figures, but Abbot certainly ought to know a great deal about the subject, and he had undoubtedly convinced himself that there was a real variation in the amount of energy given out by the sun. And if they thought of the amount represented by a difference of 10 per cent. on such an enormous quantity, it could not but be realised that this was a result of very great importance. But he thought that on the whole they had been shown that evening rather the purpose and the methods of the instruments employed in these

observations, which were perhaps quite as interesting in their way as the results obtained; and he for one was very much indebted to Mr. Whipple for the clear and comprehensive account he had given them. (Applause.)

DR. CHREE said he had been very glad to avail himself of the invitation extended to him to come there and listen to Mr. Whipple's paper. He had listened to it with very great pleasure, partly because its author was at one time, in his younger days, an assistant of his, and partly because it dealt with a subject which was naturally of interest to every meteorologist. As Mr. Whipple had said, there were really two classes of observations. First, there were the observations which were of interest to meteorologists, which consisted in determining climatic conditions. For those observations it was hardly necessary to determine exactly what the amount of energy was; what was rather wanted was a comparison of one place with another place, and of one place with itself at different times. For such a purpose what was wanted was a comparatively simple instrument, and a considerable number of those instruments in use at the same time, so that the results obtained from different places might be compared. For such observations something very much simpler was wanted than some of the very elaborate instruments they had been shown that evening, which were, of course, chiefly designed for the determination of the so-called "Solar Constant." For such investigations one did require, of course, a really complicated apparatus, and also the kind of knowledge and instincts possessed rather by a physicist than by a meteorologist.

All who had listened carefully to Mr. Whipple's paper must have been much struck by the fact that a great part of the work of recent years that appeared to be of high quality had been done in America. He regretted to say that very little had been done in this country. This

was due to more causes than one. Such work required a combination of physical knowledge, familiarity with instruments, and experience of observatory work; and it further required two other things which were not readily obtainable in this country—a plentiful supply of sunshine and a considerable amount of money. Those two requisites were more abundant in America, and that was one of the reasons why so much of this work was done there.

With regard to the more simple forms of apparatus, as far as records of absolute measurements were concerned there was a great deal to be said for the Angström pyrheliometer. This did not require a great deal of complicated apparatus, and there was no glass intervening between the strips and the sun. That was really, in his opinion, the weak point in Professor Callendar's instrument. In that instrument something like 10 per cent. of the radiation was cut off by the glass, and a good deal of complication arose from that, because of the heat received part only came direct from the sun, part from the cloud, and the amount that was intercepted depended upon the source; so that it was a much more complicated matter than might at first sight be supposed. With regard to the Campbell-Stokes instrument, which was also very simple, when he was attached to the National Physical Laboratory it was one of his duties to test Campbell-Stokes instruments, and he found that some of the health resorts which considered it very important to have a large record of sunshine had discovered that by using glass spheres of different kinds and sizes a greater or less amount of sunshine could be recorded; so that it was exceedingly necessary to have a standard glass, otherwise one would get discordant results. He thought the instruments now issued by responsible makers would agree within one or two per cent. with regard to duration of sunshine. This was remarkably good. Another very simple instrument was the

black-bulb thermometer. He must confess it was news to him that the size of the black bulb itself inside was of such importance. His experience was that the principal makers nowadays turned out black-bulb thermometers which agreed, over a range of 60 to 100 degrees, within 2 or 3 degrees Fahrenheit. It was an instrument which gave exceedingly concordant results, and also, judging by the difference between different instruments, retained its properties for a long time; so that if once they knew what its indications meant, it should be well adapted for climatic observations.

Last summer they had had an arrangement for some experiments at the National Physical Laboratory on black-bulb thermometers, from which he had hoped something definite would result; but he regretted that owing to the war or other causes these experiments had not hitherto been carried out. It must have struck everyone that the number of people anxious to make new instruments was out of all proportion to the number of people who were able and willing to ascertain the exact meaning of the records obtained with them when they were made.

Another instrument to which Mr. Whipple had referred as having a possible future, and of which he himself had had considerable experience, was an instrument designed by Mr. W. E. Wilson, but it was exceedingly to be regretted that Mr. Wilson had died before the instrument was perfected. They had had some of Mr. Wilson's instruments at Kew for two or three years, working with Mr. Wilson, and they had had them only a short time when it became apparent that the instruments differed enormously between themselves; but after prolonged trial it was found that whereas they differed absolutely, the ratio of the amounts of alcohol evaporated in different instruments was exceedingly constant, so that it was a promising instrument if only a standard type could be hit upon. Mr. Whipple had referred to a defect which he

(the speaker) was not able to confirm, namely, as to the effect upon the glass of the wind blowing over it. He had made some experiments at Kew, in bright sunshine, with the view of examining that point, but although the instrument was exposed to considerable wind, he was unable to find any marked effect. But what he did find was that the instrument was capable of working backwards; exposed to bright sunshine the alcohol evaporated and collected inside the glass where it was measured. In winter nights it was the inside reservoir that was warm and the other one that was cold; so if one had an instrument at work for the whole 24 hours, and read it in the early morning, part of what was evaporated from the alcohol bulb during the day re-evaporated during the night. Really one required to take the measurements before night-fall, and not wait until the morning. He had been perplexed at first by the results, but had experimented by applying ice round the bulb, and had convinced himself as to the cause of the phenomenon.

Mr. W. MARRIOTT said it had been very interesting to hear Mr. Whipple go back to the beginning of the history of instruments for measuring solar radiation, and to trace that history up to the present time. He had also been much interested in what Dr. Chree had just said with regard to the black bulb thermometers, and thought it remarkable that they should only have differed to the extent of two or three degrees. He (Mr. Marriott) remembered Mr. Whipple's father, Mr. G. M. Whipple, when he was Superintendent of Kew Observatory, making experiments with a number of black-bulb thermometers, and finding that the difference between those thermometers was as much as 15° . It was therefore evident that as time had gone on the instrument makers had been able to make instruments of greater precision than formerly. In the early Antarctic Expeditions, in the '40's, or somewhere about then, the errors in the

thermometers were very great indeed; in one instance, when the thermometer was subsequently tested at Kew, it was found that it had an error of 28° at -70° F; whereas now the instruments taken out by such expeditions were accurate to within two or three-tenths of a degree; so it would be seen that the observations now made were very much more likely to be correct than the old records were. Those who worked up old records found this difficulty—that they did not compare satisfactorily one with another. There was often a difficulty even with the Campbell-Stokes sunshine recorder. Of course, one must have a good instrument to begin with; but how was it to be mounted? It must be exposed in such a position that it can command the sun the whole time it was above the horizon. Otherwise a complete record would not be obtained. And possibly that might account for what Dr. Chree had said—that at certain health resorts the observers tried to put their sunshine recorders in the best possible position, and to stretch the amount of the measurement of the card as much as possible, whereas at other places the observers had been quite conscientious, and were not desirous of over-estimating the amount, and therefore the results were not quite comparable. But that was a complaint which had always been made with regard to health resorts—they wanted to get as much as they could out of their instruments, and that constituted a difficulty.

At the Royal Observatory, Greenwich, there was a sunshine recorder some years ago which gradually gave a less and less amount of sunshine, and it had ultimately to be discontinued, and consequently the record was broken. And a similar instance occurred with a recorder at Regent's Park. Mr. R. H. Curtis and he (Mr. Marriott) went down to Greenwich to see the recorder, and they found that the surface of the glass was covered with an infinite number of minute reticulated markings, and there was not

sufficient burning power for the sun to make a full record. He supposed that this was due to the action of the London smoke. If some means could be devised for ensuring the permanency of the bright surface on the outside of the glass, that would be a great help; otherwise with age it might deteriorate.

MR. F. J. W. WHIPPLE referred to the black-bulb thermometer, and said that it was a very interesting question what it was that that instrument actually recorded. In the case of instruments such as the Abbot and the Michelson, and more elaborate ones of the sort, the problem which was tackled was how much radiation was received directly from the sun; the sky-shine was regarded as a nuisance which had to be eliminated. Well, the estimation of the amount of heat received directly from the sun was of interest to the astronomer, but it seemed rather doubtful whether it was a matter of fundamental importance to the ordinary dwellers on the surface of the earth. Its effect on the climate could not be so very direct. What really mattered was the total amount of heat obtained from anywhere; it did not matter whether it came directly from the sun or whether it came from the sky. And the advantage, to his mind, of the Callendar instrument was that it did tackle that problem in a very direct way; it set out to find out how much heat was received on a horizontal surface—and he supposed they might take it that a horizontal surface was an average representation, in a sense, of the earth's surface. In the case of the black-bulb thermometer, they were not getting the amount of heat falling on a horizontal surface, and whether it was fair to say it showed the amount of heat falling on a spherical surface he was not quite certain. To expect all black-bulb thermometers to indicate identical temperatures when exposed to the same radiation seemed to him just as absurd as expecting all the Abbot silver-disc instruments to give the same thermometer read-

ings. So many degrees of temperature on the thermometer scale would correspond to so much radiation, and it seemed to him there was not the slightest need to worry because the number of degrees in two cases was not the same. It should be possible to calibrate black-bulb thermometers and certify that so many degrees of excess above the air temperature correspond to so many milliwatts per square centimeter of radiation. He very much hoped it would be possible to arrive at some conclusion of that sort, so that the black-bulb thermometer, which was eminently an instrument which could be placed at every meteorological station, would be standardised, and would become a thoroughly useful instrument. (Applause.)

MR. E. GOLD said his first observation would be with reference to the Callendar instrument. Dr. Chree had already alluded to the fact that the glass surrounding the instrument might have a different effect, or did have a different effect, on the radiations of different wave-lengths. He did not know whether any experiments had been made in connection with that instrument to determine the difference of absorption between the black part and the bright part, but that might also depend, and probably did depend, upon the wave-length of the radiation falling upon it. That might be tested, without investigating actual spectra of radiation, by letting the same amount of radiation come from a hot source at a higher and a cooler source at a lower level, to see if the effect was the same. His own impression from looking at the instruments during the night and during the day was that the effect would not be the same.

Then he had been very much interested in the American instrument for measuring solar radiation which Mr. Whipple had shown them upon the screen, because although it was in a sense a very simple matter to measure the deviation of sunshine yet, when one came to deal with it in practice, it did become a matter of

considerable difficulty. He hoped at first that the American instrument would solve the problem, but it seemed to him, on consideration, that there would be with that instrument similar difficulties to those which had been experienced in connection with others. If the sun shone feebly it would take some time for the mercury to run up to the contact, while if the sun shone very brightly the mercury would run a considerable distance beyond a contact, and when the sun went in again it would take some time to fall down again and break the contact. Thus one would get too long a record on days of intermittent bright sunshine and too short a record if the sunshine were relatively feeble.

Something had been said about the question of the measurement of sunshine at health resorts. It was necessary, not only at health resorts but also at other places, to compare the measurements made by different observers with measurements made by one observer at a central place, who was supposed to treat all cards in the same way. It was found that if that was done, and if there were sent to the observers at the out-stations comparisons between their own estimates and the estimates at a central place, after a little time they were able to make their own estimates, even where the sunshine was intermittent, agree with the standard estimates, within two or three per cent. in a particular month; and if they tried seriously and conscientiously, they would get on the average the same results as would be got by a single individual measuring at the central institute.

There was no doubt that there was occasionally some sort of deterioration in the ball of the sunshine recorder. Of course, the simplest way of testing that was to compare the duration recorded at one place with the duration recorded at other places in the same district. An instance present in his mind was that of Blackpool. At Blackpool the ball of the sunshine recorder was changed somewhere about

1899 or 1900. Previously the ratio of Blackpool's sunshine to that of other places had been going down as compared with the early years, but when the ball was changed the sunshine at Blackpool increased, and the records from Blackpool and Southport were very nearly the same, whereas formerly there had been a considerable difference between them. He was tempted, as Dr. Chree was present, and some remarks had been made with regard to this subject, to relate a discovery, as he might call it, which had recently come to his notice. Some four years ago a question was put to him by one of those people who thought a Meteorological Office knew everything—(laughter)—or ought to know everything—about the weather, writing him to say which was the finest day of the week. That was a very simple question to put, but a very difficult one to answer, and as there were no statistics available, he was afraid it had remained unanswered a very long time. However, one of the assistants in the office who had a certain amount of spare time out of office hours, and was interested in the subject, arranged the records from the Kew Observatory for a number of years according to the days of the week. Of course, one would have expected that Sunday would have come out very much the best—(laughter)—and that some day in the middle of the week would have turned out the least sunny day. And on the whole he thought that in summer Sunday had come out the finest day, but for some inexplicable reason, in the winter, from October to March, over a period of fifteen years, Friday had more sunshine than any other day, and nearly half an hour more than Saturday. That was a very considerable amount on the average. During the fifteen years Friday had something like 966 hours, and Saturday had 766 hours of bright sunshine. There were no such discrepancies as regarded the other days of the week, and he was very curious to know whether this peculiar result

had anything to do with the climate, or whether it had something to do with human peculiarities. (Laughter.) If that really represented the climate of London, then it seemed to him there was no doubt they ought to change their half-holiday from Saturday to Friday. (Renewed laughter.)

MR. R. S. WHIPPLE, in reply, thanked the meeting for the kind way in which they had received his paper. He agreed with Sir Frank Dyson in admiration for the excellent work carried out by Mr. Abbot and his colleagues in America and Algeria. We in England could only look with admiration mixed with envy at the instrumental equipment used at the Mount Wilson Observatory. There appeared to be no lack of money, and as a result it was possible to construct beautiful instruments. Nearly all the solar radiation work that had been carried out in this country had been done by private effort, especially that of Professor Callendar, and one was filled with admiration for what had been done with very slender financial support.

With regard to the question of the absorption of the radiation by the glass envelope of the Callendar sunshine receiver, there was little doubt that the majority of the short waves were absorbed by the atmosphere before they reached the earth, so that only the comparatively long waves fell upon the glass envelope, and if the envelope was made of thin glass the absorption was not a serious matter. Unfortunately, one here met a great difficulty in manufacture. Until a few weeks ago it was not possible to have these glass bulbs blown in the way in which a blower blows an *x-ray* bulb. All the envelopes had to be made from "pot" glass, that is to say, the envelope was blown in the glass house at the end of a puddling iron. As a result of this it was very difficult to obtain a thin glass bulb, and one of uniform thickness. Recently, however, a glass blower sufficiently

skillful to blow such a bulb, had been found, and we should await with interest the result of the tests on the new receiver.

Mr. J. Patterson, of the Meteorological Office, Toronto, had shown that if an Angström pyrheliometer is screened with a similar bulb to that of the Callendar sunshine receiver, the readings of the two instruments were almost identical.

He was very interested in hearing Dr. Chree's remarks about the Wilson radio-integrator. The instrument appears to have the germ of a useful pyrheliometer, and it is to be hoped that someone will undertake some further experiments with it. If the inner bulb was surrounded by a vacuum envelope, as it was suggested in the paper that the upper one should be covered, the negative effect mentioned by Dr. Chree might not take place. He was also interested to hear from Dr. Chree how well the modern black-bulb thermometers agree amongst themselves. It is to be hoped that further experimental work, perhaps on the lines of Mr. Chalmers' suggestion, could be carried out with these instruments.

ON A FLUOR-GLASS CONCENTRIC EYEPiece FOR HIGH POWERS.

By LT.-COL. J. W. GIFFORD, F.R.A.S.

Read 15th April, 1915.

A SO-CALLED "monocentric," and at the same time, I believe truly concentric, eyepiece for tele-

FIG. 1.—FREE FROM SPHERICAL ERROR.



$$R_1 = 0.462965.$$

$$R_2 \text{ and } R_3 = 0.288818.$$

$$R_4 \text{ and } R_5 = -0.288818.$$

$$t_1 \text{ and } t_2 = 0.224144.$$

$$t_3 = 0.477635.$$

$$BF = 0.687088.$$

$$(R_6 = -R_5).$$

scopic use has of late years been produced by the firm of Steinheil. But the makers have so

FIG. 2.—FREE FROM COLOUR.



$$R_3 = 0.481784.$$

$$R_1 \text{ and } R_4 = 0.201972.$$

$$R_5 \text{ and } R_6 = -0.201972.$$

$$t_1 \text{ and } t_2 = 0.22976\bar{2}.$$

$$t_3 = 0.408944.$$

$$BF = 0.56826\bar{6}.$$

$$(R_{10} = -R_9).$$

(Equivalent Focus = 1 in both cases).

Glass used.

Parramantois No. 5201.

"Barium Crown Lourd"

F - C

0.010858.

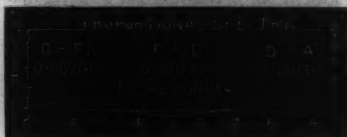
Index D 1.608728.

$$v = \frac{\mu - 1}{\delta\mu} = 58.798.$$

far apparently been unable to make one of these of shorter focus than 7 mm., for my orders for a 6 mm. in 1913 were refused.

The merits of this 7 mm. were such that in view of the short focus telescope objectives attempted in recent years, it seemed a pity that matters should be thus limited.

DIAGRAMMATIC.—I.



Fluor Glass Concentric.

An attempt was therefore made to produce a concentric eyepiece which could be made with a focus as short as 4 mm. It soon became clear, that in order to obtain spherical surfaces with radii sufficiently long for practical purposes, it would be necessary to employ fluo-rite.

DIAGRAMMATIC.—II.

RESIDUAL ERRORS (CALCULATED).

	No. 1.	No. 2.
Aplanatism	-0.00018	-0.00248
Achromatism (line D) ...	0.00088	0.00011
„ (line E) ...	0.00055	0.00018
Gauss Postulate ...	-0.00018	-0.00248

This was used in conjunction with a glass of the heavy barium crown type by Parra-Mantois, of Paris. A first approximation was then made, and finally residual errors to 3 decimal places were removed by trigonometrical trace. Two

distinct results were aimed at. No. 1, which should be free from spherical error, and at the same time as free from colour as was compatible with this freedom from spherical error. No. 2 the converse of No. 1, that is a combination free from colour and only fairly free from spherical error. The diagrams before you will show the results, and will give you the data.

These eyepieces have been successfully constructed by Mr. C. Baker, of 244, Holborn. Results in each case are such that it is really difficult to choose between them. But it may be stated that No. 1 is the larger and easier to make, while No. 2 gives the purest central image, and takes less fluorite. The field of view, though with the higher powers necessarily much restricted, is extremely flat.

For discussion see p. 202.

A REVISED SYSTEM OF COMPARATIVE OPTICS.

BY LT.-COLONEL J. W. GIFFORD, F.R.A.S.

Read 15th April, 1915.

1. THE great progress in the production of optical glass made towards the end of the last century in Germany, though in that country State-aided, depended in no small degree on the system of classification of the different types of glass produced. This system is generally, and I believe justly, attributed to the late Professor Abbe. For the purpose of refreshing your memory I will briefly run through its salient points.

A series of wave-lengths embracing almost the whole of the visual spectrum was taken, those being chosen which were suitable as well as easily produced, when both these requirements could be reconciled. But of this more anon. These wave-lengths were:—

DIAGRAM A.

K α (the mean of the red lines), 7,682 microns called A'.

H α (the hydrogen red line), 6,563 microns called C.

Na (the mean of the *d* lines), 5,893 microns called D.

H β (the green hydrogen line), 4,861 microns called F.

H γ (the violet hydrogen line), 4,341 microns called G'.

The deviations caused by interposing a prism of the glass under measurement in the path of each of these rays in turn, were carefully observed and corresponding refractive indices deduced. The spectrum was then divided into three parts by subtracting the indices, A' to D, D to F, and F to G'. A central portion from C to F, the two points then generally chosen for equal foci in achromatics, was also taken (usually called $\delta\mu$ in optics) as a measure of these three others, which were called partial dispersions. These partial dispersions divided by the central portion C to F ($\delta\mu$) were called proportional dispersions, and on the similarity of their proportional dispersions, other things being equal, depended the suitability or otherwise of two or more glasses being associated for the purpose of bringing about the reduction or absence of colour in lens combinations. A measure of the outstanding refraction in such combinations was obtained by the amount of difference shown between their components with regard to a special factor ν obtained for each glass by dividing its refractive index for D less unity by the central portion C to F, thus: $\nu = \mu - 1/\delta\mu$. (ν is the reciprocal of Coddington's dispersive power p .)

Now for a worked example. Let us take Schott's three meltings, Nos. 0,5077, 0,4805, and 0,4677. My own measurements for the refractive indices for the wave-lengths cited follow:—

No.	0,5077.	0,4805	0,4677.
A'.	...	...	...
C.	...	...	...
D.	...	...	...
F.	...	...	...
G	...	...	...

DIAGRAM B.

Taking these to 5 decimal places only, subtracting and dividing we obtain:—

No.	C - F ($\delta\mu$).	A' - D.	D - F.	F - G'	ν
0.5077 ...	...	0.00805 ...	0.00526 ...	0.00452 ...	53.64
			0.483	0.561	
0.4805 ...	...	0.01027 ...	0.00660 ...	0.00386 ...	51.55
			0.643	0.571	
0.4677 ...	...	0.01026 ...	0.00648 ...	0.00594 ...	53.50
			0.582	0.579	

The small figures are the proportional spectra. Now if we subtract those of No. 0,5077 and 0,4805 from each other we shall find from the remaining secondary spectra that these two glasses would combine to make a doublet fairly free from colour. But dividing the higher ν

factor by the lower, $\frac{63.64}{51.55}$, we only get

$1.235 \left[\frac{\nu}{\nu'} = N \right]$ for our N , and know that the

contemplated doublet would have to be constructed for a long focus in proportion to its aperture. This is shown in the diagram. In testing for a triple we proceed similarly, except that we add the partial spectra, etc., of the two glasses representing the positive components of the proposed combination, and dividing, get joint proportional spectra from the results, treating the two subsequently as one positive element, and proceeding to combine this with the negative element as in a doublet. The secondary spectrum is now entirely removed, and the outstanding tertiary spectrum negligible. It will be seen from the diagram that N is now only 1.124, but this need not trouble us, for we have two more spherical surfaces over which to distribute it, and hence a lens of comparatively short focus may be constructed.

Such is the system of Abbe and its application.

2. Now let us consider whether by venturing to modify it we may further extend its use, and make it more applicable to modern requirements. Let me here state that what I now offer is a suggestion only, and is by no means to be taken as authoritative.

Let us look first at the wave-lengths chosen. Calling them by their names as lines: A' is a difficult line to measure, the mean of two faint double lines, but we lack anything better in that

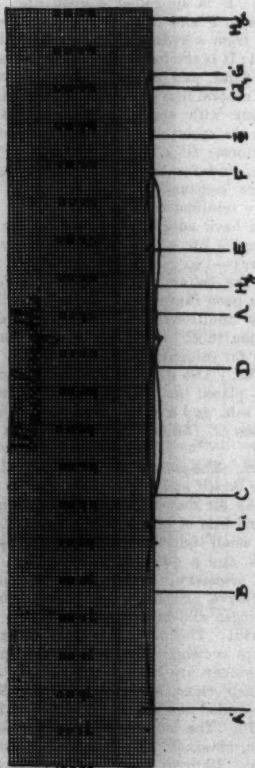


FIG. 1.—ABBE SYSTEM.

position; C is altogether a good line, and together with F and G' may be obtained at small expense from a hydrogen tube and small induction coil; D is the mean of d_1, d_2 , the lines of the sodium flame, and although good and brilliant and the central line of the five adopted, it is not coincident with the point of greatest visual effect in the spectrum; F a good line, but none too brilliant; G' a faint line, most trying to measure, and for this reason bad. All these lines are constantly called for, and I have therefore retained them in the list and for my own use have added other lines to the list of wave-lengths for which meltings are measured. These are:—

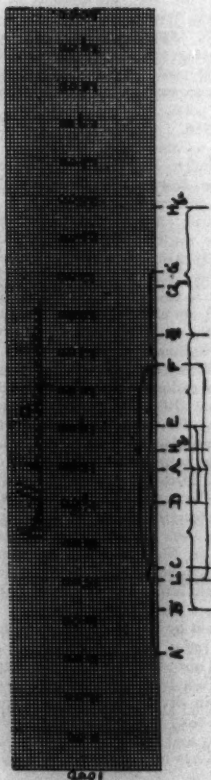
B'. The helium line at wave-length 7066. This is a good sharp line of sufficient brilliancy, the only additional apparatus required being the helium tube. It is also in just the right position for computing optical triples.

Li 6708. Also a good line, though not quite so well placed as B'. A platinum wire, a lithium salt, and a Bunsen burner, are all that is necessary. (Added at the suggestion of Dr. Lowry.)

A 5607. The point of maximum visual luminosity for myself and many others, in fact I think I may say for the majority. A most important sharp fine line of exceptional brilliancy. Lead poles, a small induction coil, say with $3\frac{1}{2}$ inches of spark, and a quart size Leyden jar, are all that is necessary. Indeed a smaller jar and coil will serve. Such a coil is necessary both for the hydrogen and helium tubes.

Hg, 5461. The importance of this line, where workshops are lighted by the Cooper-Hewitt or other mercury arc lamps, goes without saying. It is sharp, clear, and also exceptionally brilliant. (Added at the suggestion of Dr. Lowry.)

E. 5270. The well-known line in the solar spectrum, obtainable also from the iron spark spectrum. Its importance lies in its favourable



position for the calculation of achromatic doublets.

Φ 4678. Approximately the point of maximum photographic effect where ordinary glass lenses are used. A good sharp line obtainable in the same way as Λ , substituting cadmium for the lead poles.

Cd , 4415. Another useful cadmium line obtainable as above and, considering its position in the violet, of considerable luminosity and sharpness, from this point of view alone therefore far preferable to G' .

Hg_2 4046. A line in the remote visible violet, given by the mercury arc lamp, and the only practically strong line in that position.

It is not for a moment contended that all these lines are necessary, for if one of the modified systems I am about to describe be adopted, then most of the original five may be dropped.

There are many other lines which I have used for measurements, notably some excellent lines from the cadmium arc, also suggested by Dr. Lowry, but although in every other way suitable, they were, for this purpose, found not to be as favourably placed as those described.

Now let us go back to our system of classification, and discarding the original five lines, see what can be done with lines B' , Li , Λ , F , Φ , and Hg_2 . Referring to the diagrams, in No. 1 you have the Abbe system, the scale being wave-lengths; in No. 2 the same on a scale of reciprocals of wave-lengths or oscillation frequencies. This latter is very nearly the same as a scale based on the refractions of quartz, as pointed out by Dr. Marshall Watts in the preface to his "Index of Spectra." In No. 2 another system is also shown by the lower brackets, based on the lines just mentioned. The spectrum is now again divided into three portions, but in this case they are from B' to Λ , Λ to Φ , and Φ to Hg_2 , and the total portion of the spectrum embraced has been shifted higher up by the dis-

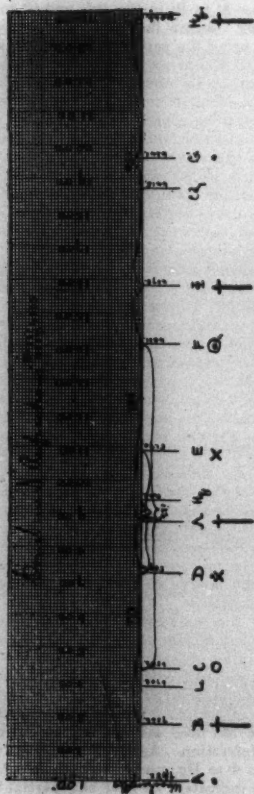


FIG. 8.—PARTIAL AND PROPORTIONAL DISPERSIONS.

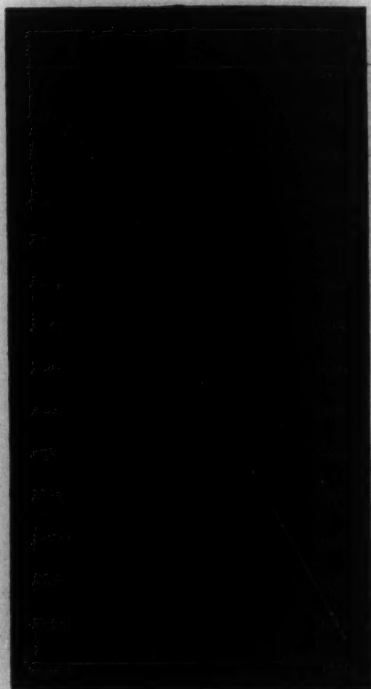
Abbe's system indicated by dots and o's.

Revised system by daggers and crosses.

tance A' to B' in the red, and G' to Hg₂ in the violet. Again instead of our measure or divisor being the portion from C to F, we now have Li to F having Λ , the maximum luminosity point, at its centre. The portion of the spectrum from Φ to Hg₂ has been added from considerations connected with stellar photography impressed on me by the late Sir David Gill.

It will at once be seen that the oscillation frequency scale, bringing as it does the parts into which the spectrum is divided by either system into their correct proportion, or nearly so, is a great improvement. In order to get a still further improvement in this respect I have constructed a scale from the mean refractions of the three glasses already referred to as forming the basis for a successful triple. This secures an average increase of the scale, and hence of each partial dispersion in turn "*pari passu*" with the increase of refraction. This is well shown in scale diagram 3, for a system similar as to partial dispersions to the one last described, but in which the measure is again the old one (from C to F); and finally when the measure is that part of the spectrum enclosed between lines D and E. To make things quite clear, in the last case the three new portions into which the spectrum has been divided are indicated at the bottom by four daggers, and the portion by which it is measured by crosses, whereas the Abbe system is indicated similarly by dots and O's, although not as in the other case outlined by brackets.

3. Now we come to the evidence in favour of this last system, the one which after long consideration I have adopted. In the first place the scale of combined refractions speaks I think for itself, for it is easily apparent that each of the three portions into which the spectrum has been divided increase in extent with the refraction. Again, if we neglect the portion from Φ to Hg₂ useful perhaps only for the purpose indicated, we have two partial dispersions



only, those from B' to Λ and from Λ to Φ . The three lines B' , Λ and Φ bounding these are those which, when their foci have been equalised, have invariably given me the most successful apochromatic triples; indeed, much more so than any combinations of foci for three other lines, for instance A' , D and G' , or Li , Hg_1 and Cd_7 . Similarly the portion from D to E used in this case as the measure ($\delta\mu$) is enclosed by the two lines which I have found most useful for equal foci in achromatic doublets, though for this I am indebted to an early paper on the Lick refractor by Mr. Dennis Taylor, who made it clear that these two points bound that portion of the spectrum naturally selected in vision. It will be seen that again we need measure five refractive indices only, this time B' , D , Λ , E , and Φ , but we now have, I claim, a more logical and definite basis for what we have done, and at the same time have instituted a decidedly more searching test by narrowing our measure from C to F to D to E . In view of the great variety of types of optical glass made in recent years, this I think is to be desired. At the same time unless the portion from Φ to Hg_2 is for special reasons required, we need deal with two partial spectra only—those from B' to Λ and from Λ to Φ and these are divided by Λ , the point of maximum visual luminosity, a much more convenient arrangement.

4. A few words on errors of observation when measuring refractive indices. Eliminating the thermometer and the barometer, there are two main sources of error. The first is incorrect collimation. A certain training of the eye is necessary, but by adopting Prof. Schuster's method and taking sufficient excursions on either side of minimum deviations, the difficulty should be removed. Another greater source of error lies in the use of unsuitable lines for measurement, such as A' and G' . With such lines, owing to their lack of luminosity, a very great number of

DIAGRAM A.

OLD LIST.

Wavelengths.

K α (the mean red line)	7682 microns called A'.
H α (the hydrogen red line)	6363 " C.
N α (the mean of the <i>d</i> lines)	5898 " D.
H β (the green hydrogen line)	4861 " F.
H γ (the violet hydrogen line)	4341 " G

REFRACTIVE INDICES.

No.	0.5077.	0.4805.	0.4677.	No.
A.	...	1.52284	...	1.54247
C.	...	1.50860	...	1.52641
D.	...	1.51229	...	1.52944
F.	...	1.51794	...	1.53068
G'.	...	1.52246	...	1.54254

NEW LIST.

Wavelengths.

He (the red helium line)	7066 microns called B'
Na (mean of <i>d</i> lines)	5898 " D.
Pb (a lead line)	5607 " A.
Fe (and solar spectrum)	5270 " E.
Cd λ (a cadmium line)	4678 " Φ .

REFRACTIVE INDICES.

No.	0.5077.	0.4805.	0.4677.	No.
B.	...	1.50848	...	1.52462
D.	...	1.51239	...	1.52944
A.	...	1.51854	...	1.53105
E.	...	1.51590	...	1.53328
Φ .	...	1.51985	...	1.53852

DIAGRAM B. OUTSTANDING COLOUR.

SYSTEM OF ABSE.

Doublet (Achromatic).

$\mu_D - 1.$	$C - F.$ ($\delta\mu$)	$\nu = \frac{\mu - 1}{\delta\mu}$	A' - D.	D - F.	F G'	No.
0.51229	0.00805	63.64	0.00526 0.0008	0.00585 0.702	0.00452 0.561	0,5077
0.52944	0.01027	51.55	0.00860	0.00724	0.00586	0,4805
$N = \frac{\nu}{\nu^1} = \frac{63.64}{51.55}$		1.235.	$\frac{-0.643}{0.453}$ -0.000	$\frac{-0.705}{0.702}$ -0.005	$\frac{-0.571}{0.561}$ -0.010	

Triple (Apochromatic).

0.54895	0.01028	57.98	0.00648	0.00725	0.00594	0,4677
0.51229	0.00805		0.00526	0.00585	0.00452	0,5077
1.06124	0.01881		0.01174	0.01290	0.01046	
$N = \frac{\nu}{\nu^1} = \frac{57.98}{51.55}$		1.124.	$\frac{0.641}{-0.443}$ -0.005	$\frac{0.705}{-0.702}$ 0.005	$\frac{0.571}{-0.571}$ 0.000	0,4805

REVISED SYSTEM (TRIPLE).

$\mu_A - 1$	E - D.	$\nu = \frac{\mu - 1}{\delta\mu}$.	B' - A.	$\Lambda - \Phi$.	Φ Hg ₂ .	No.
0.1354	0.00802	154.7	0.00505	0.00581	0.00651	0.5077
0.55056	0.00886		0.00637	0.00751	0.00862	0.4877
1.06410	0.00888	188.1	0.01142	0.01332	0.01513	0.4805
0.58105	0.00885		1.660	1.385	2.190	
			0.00848	0.00747	0.00857	
			-1.670	-1.940	-2.226	
			1.660	1.385	2.190	
			-0.010	-0.004	-0.027	

$$N = \frac{\nu}{\nu^1} = \frac{154.7}{188.1} = 1.120.$$

CORRESPONDING DATA FOR DOUBLET.

$N = \frac{\nu}{\nu^1} = \frac{170.8}{188.1} = 1.333$	outstanding colour	0.077	0.4805
		-0.005	

observations have to be made in order to attain the lowest standard of accuracy.

In order to make sure that my own refractive indices of glasses are sufficiently correct for all optical purposes independently of any doubts which might arise as to accuracy of wave-lengths, Mrs. Gifford has drawn curves in which instead of wave-lengths for the co-ordinates in the curve, the refractive indices of another of my glass meltings have been substituted. Two such curves are before you, due to the three glasses already referred to. In both cases the co-ordinates are the indices of 0.5077 borosilicate crown. In the one case the ordinates are the indices of 0.4805 borosilicate flint, in the other of 0.3187 baryta light flint. It will be noted that the curves are practically straight lines, but that the slight curvature such as it is is regular.

In conclusion, let me again say that I claim no authority in these matters. I have only thought to interest you by describing the arrangements I have adopted for my own use.

Discussion on "Fluor-Glass Concentric Eyepiece for High Powers."

MR. T. SMITH said that from the title of the paper he had imagined that Col. Gifford had succeeded in constructing an eye-piece with fewer residual errors by employing fluor crown glass, one of the most recent products of the Schott glassworks, and he had hoped to get from Col. Gifford some information about that glass. But having now heard the paper, he thought the questions he had intended putting would hardly be to the point. It seemed to him a very notable achievement on the part of Col. Gifford to have got out an objective as good as that of which he had told them with such an extraordinarily short focal length, and he thought many or those present would like to have the

opportunity of trying the eye-pieces for themselves.

THE PRESIDENT asked whether Col. Gifford had tried the resolving power of the eye-piece in bacteriological work. There were many questions yet to be settled regarding the morphological structure of bacteria, because the resolving power of existing microscopes was insufficient for the purpose, and with an eye-piece such as Col. Gifford had described it might be possible to obtain more detailed information as to their structure. He also asked whether Col. Gifford could tell them something with regard to the homogeneity of fluorite, and as to its optical workability.

DR. WALMSLEY said it was evident from the figures on the screen that Col. Gifford's calculations had been carried to a very high degree of accuracy. The number of the figures of the radii were simply staggering as regards the minutely small quantities with which they deal. Now if those figures were multiplied by so much as 4, how close to them did Col. Gifford expect the manufacturer to work?

Reply.

COL. GIFFORD, in reply, said he was afraid he had treated Mr. Smith very badly. He had come to the room that evening furnished with all his papers and diagrams, but had entirely forgotten to bring the eye-pieces themselves. (Laughter.) However, he had fortunately had time to return to his chambers and fetch them, and he now had pleasure in handing them round for inspection. In regard to Dr. Ettles' question concerning bacteriological work, he saw no reason why the eye-piece should not be used in conjunction with a micro-objective; but it would not add any extra resolution. However, with an oil immersion of sufficiently high aperture, no doubt it could be used with advantage. Fluorite is homogeneous, and being soft is fairly

easily worked. Mr. Last had asked a question with regard to the flatness of the field. He had not measured the curvature. It was impossible to have a perfectly flat field—it was necessarily a curved field—but it was so flat that it was possible to see a star right to the edge, and to follow a star right across the field. That was especially so with the first of the two; in the case of the second it was not so much the case, as it was necessary to avoid the extreme edge, but even then it went very close to the edge. In reference to the point raised by Dr. Walmsley, he had not been aware of any defects in the corrections, except perhaps a few in regard to the centreing. In his experience the accuracy of the manufacturer had not exceeded four figures.

Discussion on "A Revised System of Comparative Optics."

Mr. J. W. GORDON, K.C., said he was very much obliged for the opportunity of making a few observations on Col. Gifford's most interesting paper. One point puzzled him much, no doubt owing to its novelty. Col. Gifford had said that he obtained a very much more searching analysis of his results by using the limited area from D to E, instead of the more extended area from C to F for the basis of his calculations. He (the speaker) should have thought that what was wanted for the purpose of what he might call a ground-work comparison was to have a standard of comparison which would substantially cover the whole region, or at least be representative of the whole region, to be explored. He might perhaps be allowed to illustrate his difficulty by putting an extreme case. Suppose that Col. Gifford, instead of taking the limited area from D to E, had limited himself to a very

narrow area which might be compared to a particular line. It was quite clear that, in that case, he would lose the benefit of having comparative magnitudes to deal with at all. He (the speaker) should have thought that it was impossible to cut down the common denominator of all the factors without to an extent incurring the same kind of lack of significance which would result from pushing the restriction to extremes. That was the way in which it struck him off-hand, though he was quite sure there was some answer to that objection, and that there was a very good reason for Col. Gifford's saying that by restricting the common denominator area he obtained a more searching analysis. He quite saw that in that way he obtained larger quotients, and it might be that he so secured results which spoke more to the imagination. But he (the speaker) did not suppose that was what Col. Gifford meant. There was no doubt some reason why the use of the restricted common denominator gave a more searching analysis, but that reason was not intelligible to him at the moment. Another thing he wanted to say was this. He had no doubt he was expressing everybody's feeling when he said that it certainly did strike one that this method of dissecting up one's evidence, and selecting the parts which were to be endowed with individuality, was a distinct improvement. One would think, merely from looking at the diagrams which Col. Gifford had brought before them, that must be so; and they all knew how great was Col. Gifford's experience of these matters. Speaking for himself, he felt it behoved him to form opinions on such a matter only with the greatest reserve, but his opinion in this instance was strengthened by bearing in mind Col. Gifford's immense experience and immense industry in regard to this particular subject. Col. Gifford had certainly succeeded in stating with great clearness the merits of his system, and he thought that when once he had elucidated the advantage of reducing the dimen-

sions of his common denominator, he would have made the whole subject perfectly clear.

THE PRESIDENT said that Col. Gifford would like to answer each question while it was fresh in the minds of the audience, and he thought it would perhaps be a good plan to depart from their usual practice in this respect. (Hear, hear.)

Reply.

COL. GIFFORD said that what was at the bottom of his mind in choosing D. to E was that this was the part of the spectrum which was selected by the eye, if what Mr. Dennis Taylor said some years ago was correct when speaking about the Lick Refractor. He considered that the point of the spectrum which was chosen *per se* by the eye was that portion lying between those two lines, and whatever the light was, daylight or any other light, the eye would seek that part of the spectrum and that alone, and that part of the spectrum would govern everything else. That was why he (the speaker) had chosen that to be a measure of, all the rest. He could better explain, perhaps, to Mr. Gordon by showing him his papers. He had drawn up a list of all the glasses arranged on this new system, and had set out what the tertiary spectrum was both on the ordinary Abbe system and on the new system. It was quite true that the result was much greater by dividing by D and E than by dividing by the portion from C to F.

THE PRESIDENT said he supposed it was a matter of no particular interest in the present discussion that the most luminous portion of the spectrum shifted. When the spectrum was diminished in luminosity the eye selected a different place. When the spectrum was very brilliant, the portion near the D line was chosen as the most brilliant in ordinary illumination, but when the slit was shut down, and the luminosity of the whole spectrum was reduced to

minimum visibility, then the most luminous portion came in, which differed from the yellow to the green. Still, he did not think that affected Col. Gifford's point, which applied to ordinary illumination.*

DR. LOWRY said he was only a visitor to the Optical Society, and what he could say about optics was perhaps hardly worth saying. He looked at everything from the point of view of polarimetry, and from that point of view the question arose, What were the most suitable light sources to use? In polarimetry the point of supreme importance was to secure the maximum of illumination, and in the course of his polarimetric work he had arrived at the selection of something like eight principal lines. Of these the dominant line was the green mercury line. This was an absolutely magnificent line, and now that commercial mercury arcs were so easily procured, he believed it would steadily displace sodium for all ordinary monochromatic work. In all his own work for the last eight years he had put the mercury line in the very forefront, as being the purest, brightest, and easiest line to read. The other line which was supremely important was the mercury violet line, which was not shown in Col. Gifford's list. One was compelled to use that line in polarimetric work. The reason was that all the curves of rotatory dispersion ran up very steeply in the violet region of the spectrum, and it was very important to get the violet line as bright as possible. The question arose whether that line might not be substituted for G in measuring refractive indices. The drawback to the line was that it had two weak

* *Author's Note.*—Line D is in the yellow. Line E is in the green. What Dr. Ettles has said merely confirms what I have said as to the advantage of my choice of the region D to E as a measure of the spectrum.

satellites, but in using the refractometer that made no difference at all, as both satellites were of shorter wave-length than the main line. In addition to the two mercury lines, it was necessary to have some other lines to check the form of the dispersion curves. For a long time he had been using the sodium and lithium lines, but he had been very glad to abandon the lithium line because it was the worst line of the whole series to read. In addition to, and in place of, the two lines he had just mentioned, he had been using the cadmium red line, which was a fine line of extraordinary purity, very much purer than the mercury green; in fact, it was probably one of the very purest lines known. He also used the cadmium blue and the cadmium green, and in his opinion these three cadmium lines would have to come in for every-day use. Then the question was, having settled the best system for polarimetry, how many of these lines would be used for other optical purposes? He agreed with all Col. Gifford had said in condemnation of the hydrogen lines, and would gladly co-operate in any attempt to replace them by others; but it was not at all certain that the best standard lines for use in designing lenses would be the best for polarimetry, or *vice versa*. His object in putting forward his own series of lines was to secure co-operative action when (and only when) it appeared to be advantageous, as, for instance, in measuring the refractive power of organic compounds.

There was one additional point he would like to press very strongly at the present time. The optical glass industry in Germany owed very much to the scientific reputation of Zeiss, which provided a most valuable advertisement for the commercial products to which his work had been applied. As a matter of pure business, he believed it to be of the utmost importance that English firms should take up Col. Gifford's

work and see that English interests secured the commercial advantages that might be derived from the reputation which that work had gained all over the world. The publication of a catalogue of optical glasses in which refractive indices were given for Col. Gifford's series of lines would constitute a claim that the optical glass industry in England was definitely in advance of those interests which still used only the old Fraunhofer series of lines.

Reply.

COL. GIFFORD said that with regard to the special application of these measurements to polarimetry, he had not taken that into consideration at all in drawing up his list of wave-lengths. His system of classification was merely intended for ordinary optical work. With regard to the suggestion that line 4358 might replace line 4341, his objection was not that it would not replace it, but that that line was not in the best place for the purpose in view. It was his experience that the three points which were absolutely the best were Φ , Λ , and B' . That was his only objection to the line Dr. Lowry had spoken of, which was excellent from every other point of view.

MR. T. F. CONNOLLY said it was not quite clear to his mind that the slight inaccuracies in the existing wave-lengths would have the same effect in the two glasses. He could quite imagine that in the case of a pure and very uniform sample of glass very small discrepancies in wave-lengths might be recognised. There was one small criticism which perhaps he might be allowed to make, and that was that it was usual in the case of spectral diagrams to show the violet on the left and the red on the right, and perhaps Col. Gifford would agree with him that that might be a desirable change to make in his diagrams.

Reply.

COL. GIFFORD said that what Mr. Connolly had observed with regard to errors to be found in glass was quite true. It was almost impossible to get optical glass which would even in the same block of glass be uniform beyond the fourth decimal place. He had drawn attention to that in a paper he had previously published. He was afraid he had not quite understood Mr. Connolly's second point, with regard to the spectra.

DR. ROSENHAIN said the suggestion put forward by Col. Gifford was not only a very interesting one, but one for which the optical world had been waiting for some time. Nearly ten years ago, when the Optical Glass Committee of the Optical Standards Committee was in active existence, that question had been raised, and a suggestion had been put forward that the green mercury line should be adopted as the standard for specifying the refractive index. He had had the privilege at that time of going to Jena and discussing the whole question with the late Professor Abbe, and although that was a good many years ago, it might be interesting to recall the Professor's views. They were these: that it would be difficult to change to other lines of the spectrum, owing to a large extent to the hold which the old ones had got—that it would be a step requiring international agreement to begin with, because it was not everybody who could remeasure the optical properties of their glass before proceeding to use it. In the ordinary course of things, one had more or less to rely on the figures given in the catalogues of the makers, and he did not think the glass makers would be very willing, or very anxious at any rate, to give a list of refractive indices occupying some 20 or 30 lines. That being the case, a whole crop of questions came up for consideration, and one of these was as to ease of production and another as to ease of identification of various lines. It

was all very well for an expert like Col. Gifford or Dr. Lowry to identify all these lines without risk of error, but a great many measurements of this kind had to be made by people who were not acquainted with more or less unusual spectra like cadmium, helium, and so on, and there was therefore every chance of their mistaking one of these lines for another. He had seen hydrogen tubes in which there was a certain amount of mercury vapour, and there was quite a difficulty in making up one's mind as to which of the lines near G' being dealt with. They were all difficult to see, and at times one was quite unable to see any of them. He could assure them from his own experience that often one was by no means as certain as it was desirable to be that one was really measuring the right line. It was perhaps more a question of choosing one's sources of light, but all that meant complication, and to a certain extent difficulty. One of the difficulties about the G line and a line anywhere near it was the fact that light of that wave-length had great difficulty in getting through certain glasses at all. In the case of the denser flint glasses, for instance, they were slightly yellow, and it was extremely difficult to see those lines through them. A great many of them were obliged to use the Pulfrich refractometer, and there was considerable difficulty in getting those short wave-lengths to go through at all.

DR. LOWRY: There is plenty of light through the mercury.

DR. ROSENHAIN, continuing, said that in the days when he was doing this kind of work the mercury arc was not so available as it was now. But that again emphasised the point that everything depended upon the sources of light available. That some more rational subdivision was desirable he thought Col. Gifford had made quite clear, but the whole question was, what were the lines to be chosen, and how were they to be produced. The considerations he (the speaker) had

urged were, he thought, worth bearing in mind. It would not do to take too high-and-mighty a view, and to say, "You ought to know how to produce them, and you have got to do it." Optics was to a large extent a commercial matter, and the practical feasibility of any suggested methods, in the hands of people who were not excessively skilled, had to be considered. None the less, the whole matter was very important, and he thought the Society should be very grateful to Col. Gifford for having brought it before them.

Reply.

COL. GIFFORD said that with regard to the line 4341, he was glad Dr. Rosenhain had "hammered" that, because that line had given him more trouble than any other. As Dr. Rosenhain had said, the mercury line was apt to get in, and one did not know what one was doing. Besides which, one was likely to get a pretty good headache the next morning. (Laughter.) That was why he had substituted the line 4678. It was produced quite easily, and it stood alone by itself; it could not be mistaken for any other. And similarly with the other lines he had chosen, he thought he could conscientiously say that he had had in view in every case that they should be lines which could not be mistaken. Perhaps he might add one thing which he had omitted to say in replying to Mr. Gordon with regard to D and E. He was only following the best method in choosing D and E, because D and E were now the best lines to work a doublet on whereas formerly C and F used to be the best lines.

MR. LAMB said that he, like Dr. Rosenhain, had had a certain amount of difficulty in dealing with the G' line. He thought it would be a good thing to have a system such as Col. Gifford had put before them that evening, utilising lines very much more definite, and capable of being read with greater accuracy

than those adopted at the present time. No doubt difficulties and complications would arise, because one manufacturer may place in his catalogue readings of lines such as Col. Gifford had mentioned, and another retain the old notation. It would mean that all manufacturers would have to fall into line and work according to the same method. As far as the general optician was concerned, he thought the lines at present given, C, D, F, G, suited his purpose very well, but when one came to do such work as that on which Col. Gifford was engaged, one had to be extremely accurate. He thought that the people engaged on such work would prefer themselves to determine the optical constants to the finest possible degree of accuracy. However accurate the glass maker supplies his readings, people of scientific attainments like Col. Gifford would only rely upon the readings they obtained themselves, because the variation of the optical constants even in a few inches of the same melting of glass was quite an important factor. In making such very fine calculations as those put before them by Col. Gifford one would not be able to rely upon the makers entirely, because a difference of even one in the fourth place of decimals would be a matter of importance.

Reply.

COL. GIFFORD remarked that he was very pleased at what Mr. Lamb had said. Of course he had known all along that glass at the present time was not being produced anything like up to the accuracy that he had been dealing with. The great difficulty appeared to be to get one part of the melting homogeneous with the other part. Generally his system had been to choose three blocks from different parts of the melting, to cut these into prisms and measure the three, and then, taking the mean probable error of the three, to find out really what errors he had to expect. But he was rather living in hopes that now Messrs. Chance had taken up the matter

of producing glass of this description in earnest, some good results would follow. They all knew what had been done in Germany, and he, like a great many others in this country, was literally ashamed of the way in which the Germans had got past them in this matter. He, and those who worked with him, had been trying and struggling to do all they could, and now that Messrs. Chance were doing the same, they might at length attain the degree of accuracy that they had been talking about. He only hoped it might be so.

Mr. LAST said it was not altogether a matter which rested with the glass maker. There must be co-operation between the glass maker and the user of the material. In this connection he would remind them of the co-operation between Schott and Abbe, and he thought the work of the latter was too often forgotten.

Mr. T. SMITH said that his experience in making measurements of glass, and of the difficulty of using some of the lines, especially the G' line, had been exactly similar to that of previous speakers. He had found that the mercury line, 4358, was a most useful one to fall back upon when any difficulty was found in seeing the G' line, and it was possible if the G' line was not easily seen, to use the mercury line and apply a correction. He thought the standard lines A' , C , D , F , and G' , with the mercury line to fall back upon, were very suitable for ordinary commercial purposes. The measurements on those lines happened to be exceedingly convenient from the point of view of checking the accuracy of results; it could be seen almost in a moment whether one's results were correct within about one in the third figure in the ratios of the partial dispersions.

The fact that such checks could be applied suggested that the refractive index of any ordinary glass could be obtained for practically any line, by means of a very simple formula when the values of μ_D and γ were known. He had not

tested the formula over the series of lines Col. Gifford had used, but hoped to do so quite shortly. One consequence of the one formula being applicable to practically all ordinary glasses was that, unless one used an exception glass, such as borosilicate flint or one of the kinds of glass that manufacturers had practically cut out of their lists as being insufficiently permanent, one obtained exactly the same secondary spectra for telescope objectives or thin achromatic lenses of different glasses.

Reply.

COL. GIFFORD said he was afraid that with regard to the absolute similarity, or identity, of all secondary spectra, he could not quite agree with Mr. Smith's remarks. He did not quite follow Mr. Smith with respect to his formula for ascertaining the refractive index for any line whatever from one given refractive index; indeed, it had not been his experience that the effective classification of glasses depended entirely on the dispersion, not on the refraction, and that no formula was necessary. He had arranged the glass meltings in his tables in columns in ascending series of their dispersions,* and then, by interpolating between corresponding elements in contiguous columns, it was easy to find the index for any glass whose optical position lies between those of the glasses to which the columns correspond. Mrs. Gifford has taken this up, and finds it liable to an error of unity in the fourth decimal place only.

THE PRESIDENT conveyed to Col. Gifford the formal thanks of the Society for his two very able and most interesting papers. He was sure the second one would prove to be of great practical value, especially at the present time, when the nation was looking for constructive talent amongst its sons, in order to see what could be done to render England independent of outside sources in regard to its scientific work.

* Roy. Soc. Proc. A, vol. 87, 1912, p. 190; and vol. 91, 1915, p. 320.

THE DESIGN OF SEARCHLIGHTS AND
PROJECTORS AND THE VISIBILITY OF
DISTANT ILLUMINATED OBJECTS.

By Mr. J. S. Dow.

Read 13th May, 1915.

It is some years since I last had the pleasure of lecturing before the Optical Society. On that occasion I dealt with photometry.

It occurred to me that, on this occasion, some of the points discussed at two recent meetings of the Illuminating Engineering Society devoted respectively to searchlights and visibility* would be of interest. The lecture falls naturally into two parts: (1) that dealing with apparatus for illuminating distant objects, (2) that relating to the appearance of the distant objects thus illuminated.

A broad distinction may be drawn between "beacons" and "searchlights." Beacons are luminous objects visible to ships afar off. Searchlights illuminate the ships so that they may themselves be seen. In the case of beacons the light is usually emitted over a wide angle, so as to be visible from many different directions; in the case of searchlights the beam is confined to narrow limits, with a view to obtaining a maximum brilliancy in one direction. Practical considerations limit the beam to about 2-3 degrees dispersion. It is not possible, in practice, to get a parallel beam. However there are few cases in which it would be desirable to

* Illum. Eng., Feb. and May, 1915.

do so, and from a practical standpoint a divergence of 2-3 degrees is quite satisfactory.

For a beacon the chief requisite in the illuminant is a high candle-power. The source may occupy a fairly large area, in some cases preferably so. But in a searchlight it is essential that the light should be concentrated over a small area, and the "intrinsic brilliancy" of the source is the most important factor. Hence it comes about that the electric arc, whose crater is estimated to have an intrinsic brilliancy of the order of 200,000 candles per square inch, is incomparably the best illuminant for a searchlight. For beacons, on the other hand, incandescent oil has advantages, while for portable searchlights and signalling lamps of small range, the oxy-acetylene light (in which a mixture of oxygen and acetylene is used to render incandescent a small pastille of ceria), and small voltage electric incandescent lamps may be used. The oxy-acetylene system is said to be extensively employed in the Swedish army. It is also possible that the new compact "half-watt" lamps will have useful applications in this direction. Very compact searchlights using the oxy-petrol system, (also operated with a pastille of ceria) have also been devised. Some of these forms of apparatus can be carried on the back, and the system is essentially self-contained, and can be set up anywhere.

For long-range work, however, the arc light is essential. The arrangement now invariably employed is to use horizontal carbons, the crater being directed towards the parabolic mirror, and situated at its focus. The negative carbon is made as small as possible so as to cause only a small shadow. I do not propose to enter into the electrical and mechanical arrangements for controlling the arc. From the optical standpoint interest centres chiefly on the source of light and the mirror. The manufacture of these mirrors has now reached a high stage of perfection, and the chief opportunities for improve-

ment seem to lie in the development of the arc source. Few people realise the enormous current taken by modern high-power searchlights. The larger sizes consume as much as 150 amperes, and the mirrors must be designed, not only with a view to perfection from the optical standpoint, but so as to stand the great heat generated and neither crack nor undergo deformation. Among various accessories recently introduced may be mentioned the use of a twin searchlight, so that one light can be kept burning while the other is recarboned. In some cases an iris shutter is used for signalling work, and this can now be operated from a distance. Arrangements can also be made for the observer to carry out observations through a telescope, which is so connected with the projector that the latter continuously follows the movements of the telescope; in this way the object looked at is always kept in the light of the beam. This is an important step, in view of the fact that observations are preferably made from a point not immediately beside the searchlight, so that the observer is not dazzled by the luminous haze in its proximity.

The Inverse Square Law.

The candle-power obtainable from these huge searchlights is enormous. With a 150 amp. projector, using a 150 cms. diam. mirror, as much as 180,000,000 candles are said to be obtained. There is a widespread impression that from a physical standpoint a searchlight cannot be said to possess any "candle-power," because of the concentration of the beam. This view, however, does not appear to be correct. If the arc were an exact point-source, so that, placed at the focus of a parabolic mirror, it produced an exactly parallel beam, the ordinary definition of candle-power according to the inverse square law would presumably break down. But in practice, as has been remarked, the dispersion always amounts to about 2-3 degrees,

and tests have shown that the illumination falls off at various distances in very close accordance with the inverse square law. The same applies to tests of motor-car and locomotive headlights.

Certain difficulties in the photometry of searchlights are introduced by the absorption of the atmosphere, and the unsteadiness of the arc. These difficulties, however, are by no means so great as is sometimes supposed, even in our uncertain climate. One would naturally choose a clear night for making tests, and in these circumstances fair results have been obtained. The matter seems worth investigation. At present the conventional method of rating searchlights is in terms of the current consumed, the diameter of the mirror, etc. Logically it would seem desirable to specify the candle-power available on a clear night, and in view of the modern improvements in instruments for measuring illumination, this method of rating should not be overlooked.

Another factor which is often stated in very vague terms is the "range" of a searchlight. This, again, naturally depends on the clearness of the atmosphere. Atmospheric absorption does not appear to have been very closely studied in this country, but an elaborate set of observations has been carried out by the Lighthouse Dept. in France, under the supervision of Prof. A. Blondel, who has also contributed an exhaustive scientific investigation into the various factors on which the range of a searchlight depends.* It is obvious that such considerations as the size of an object illuminated, its colour or tone, and the nature of the background must be considered, and that the acuteness of vision of the observer (whether with the naked eye or when using field glasses) should

* *Illum. Eng.*, Feb. and April, 1915, pp. 85 and 153.

be borne in mind. (It is related that a certain Eastern Power, when ordering a searchlight, specified that it should be possible "to see a chicken five miles away" . . . naturally an impossible feat with any illumination!)

10,000 Yards Range

Logically, the range of a searchlight is the best criterion of its practical utility, and it is well worth consideration whether some standard form of specification, taking into account these various factors, could not be contrived. Speaking generally, it is estimated that, on a clear night, and setting no limit to the size of the object illuminated, the range of the most powerful modern searchlights approaches 10,000 yards. As this is approximately the height of the Himalayas, it should serve for all practical purposes.

Before proceeding to consider some of the applications of searchlights, reference should be made to one most interesting development, the searchlight with alcohol-cooled electrodes, which was described by Mr. J. Eck and Mr. T. E. Ritchie at a recent meeting of the Illuminating Engineering Society (*loc. cit.*). It was previously supposed that the limits of improvement in the direction of efficiency of the arc had been reached with the enormous currents used in modern searchlights. In these circumstances as much as 6-7 candles per watt. may be attained. But with the alcohol-cooled electrodes it is stated that approximately four times as much light as that previously secured with a given current consumption can be obtained. In this form of searchlight the electrodes are continuously sprayed with alcohol, and are automatically rotated so as to be cooled evenly all round. The negative electrode is not horizontal, but is tilted slightly so as to cause a minimum obstruction of light. The positive carbons burn always evenly so as to form a square end, and a much larger proportion of this terminal surface is

occupied by crater. In addition, owing to the fact that the current density can be increased to about five times that ordinarily employed, the intrinsic brilliancy of the crater is much greater than that in the ordinary arc. Another point of interest is the steadiness of the arc. It appears that an ordinary arc is apt to be broken when heavy guns are fired, and has to be restarted. Naturally, it is a drawback for the arc to go out in this way, as the object on which the searchlight is playing may change its position and have to be searched for again, and possibly the range altered. The cooled electrode arc, on the other hand, is said to be able to stand a Dreadnought broadside without the arc going out.

The High-Pressure Lamp.

Another recent development is a form of arc invented by Lummer, of Breslau. It is stated that by enclosing the arc in a glass chamber and pumping in air under high pressure a much greater current density can be used, and the intrinsic brilliancy vastly increased. It appears, however, that the practical difficulties of this method are considerable, and at present it seems a less promising method than the use of cooled electrodes.

I will now pass on briefly to review some of the applications of searchlights. On warships their utility is, of course, well known. It has also been suggested that merchant ships should be equipped with searchlights, as the beam would be useful in locating distant vessels and avoiding collisions in a light fog. In a dense fog, as is well known, searchlights are not of much value. In the event of an accident at night searchlights might also be of value in illuminating the adjacent water, and enabling the boats to observe struggling survivors. Some searchlights are fitted with dispersing lenses, which, when interposed in the path of the beam, give a scattered illumination very suitable for

this purpose. In the United States searchlights have been widely used for spectacular lighting, illuminating the exteriors of buildings, etc. At the Panama-Pacific Exhibition, now proceeding, this form of lighting is being much employed. When a wide area has to be lighted prismatic lenses having a wide angle of dispersion are again very serviceable. Some screens of this kind, giving a dispersion of nearly 100 degrees, were recently described by Mr. T. E. Ritchie.

The same qualities which render the arc best adapted for searchlights also fit it for use in projectors for cinematograph work. In cases where electricity is not available the limelight, or the oxy-acetylene or oxy-petrol systems, may be used, but the light is naturally far less powerful. Few people realise how vast is the waste of light in an ordinary projector. It may be pointed out that to illuminate a ten-foot square screen with an illumination of two-foot candles (a somewhat high value in the picture theatre), only 200 lumens, *i.e.*, *less than 16 spherical candle-power*, are, theoretically, necessary. In practice an arc producing several thousand candle-power, and consuming a corresponding amount of electric energy, would be used. The efficiency of the apparatus is thus less than one per cent. Here is surely a great opening for the inventor!

Useful Range as Cube Root of Candle-Power.

The conditions affecting the range of searchlights also apply to motor-car and locomotive headlights. According to some researches by Messrs. C. C. Paterson and B. P. Dudding, the useful range of such lights is approximately proportional to the cube root of the candle-power. In view of the special regulations imposed by the authorities in war time, the question of the best means of defining the range or power of such lights is worth consideration. This would seem to be a matter for the expert, working in co-operation with the authorities, to determine,

and it has been often suggested that the existing regulations on this point are not sufficiently definite. Clearly, it would be a danger for lights on vehicles to be entirely extinguished, especially in view of the darkened character of the streets. The problem for the motorist is to strike a mean between too much and too little light.

In most cases where searchlights are employed we have a double problem: the value of the beam to the person using it, and its effect on people who are illuminated by the rays. A powerful motor-car headlight, however helpful to the motorist, has a decidedly dazzling effect on pedestrians, and this is one reason for advocating reasonable restrictions of brilliancy at the present time. The glare from searchlights has been repeatedly utilised in military operations. Searchlights have in some cases been deliberately used to dazzle the attacking enemy; and similarly in naval operations a searchlight playing on the shore may prevent the defenders from detecting landing parties in boats passing in the dark spaces between the beams.

The coming of the airship and aeroplane have brought into being a new application of searchlights, which must now be adapted to scour the sky. There are several methods of securing a vertical beam. The barrel of the projector, as a whole, may be rocked. The carbons may occupy a vertical position immediately under a lens—this avoids the danger of hot particles of carbon falling on and damaging the mirror—or the searchlight beam may be reflected upwards by means of an inclined 45 degrees mirror. In spite of a certain loss of light, this seems the neatest solution, as such a mirror can be readily manipulated so as to direct the beam in all possible directions. Another novel problem is the design of range-finders for use with anti-aircraft guns. It is well known that it is vastly more difficult to get the range of aircraft than it is to get the range of objects on land.

and there seems room for ingenuity of inventors in connection with this interesting optical problem.

Physiological Problems.

I will now pass on to the second section of my lecture—the appearance of distant luminous or illuminated objects. This twin aspect of the subject is mainly physiological in character; while the problems relating to the illuminating apparatus, whether searchlight or motor-car headlight, etc., are essentially physical. These matters are now of more than academic interest. In modern warfare *vision* plays an essential part. On sea, on land, and in the air combatants are constantly observing each other at long range. There is a constant struggle between the attacker who is attempting to locate his enemy, and the defender who endeavours to conceal his whereabouts. It is, therefore, of interest to study the appearance of distant objects.

I will deal first with distant luminous points, such as signal lights, beacons, etc. The physiological phenomena concerned are highly complex. It is well known that in ordinary circumstances the apparent brightness of such an object is approximately constant, irrespective of the distance of the observer (except for atmospheric absorption). A person walking down a street on a clear night finds that the street lamps have all about the same brightness. As he advances the amount of light entering the eye from each lamp increases according to the inverse square law; but at the same time the size of the retinal image increases to a corresponding extent, so that the amount of light per unit area, on which the mental impression of brightness is based, remains the same.

But in the case of distant faint lights this relation no longer holds good, and, according to some authorities, the physiological impression is proportional to the total light emitted

from each source. An important element in such circumstances is the colour of the light. So long as a faintly luminous point is observed with the central portion of the retina (by direct vision), a red light is seen more easily than a green or blue one; but when the luminous object is observed by oblique vision, "out of the tail of the eye," a green light is most easily seen, and, on the border of invisibility, it appears not green, but *white*. For very faint lights the peripheral region of the retina is most sensitive, but the analysis of colour is best adapted in the central region of the retina. Therefore a red light, so long as it is visible at all, will appear red; but a green light, seen by oblique vision, loses its colour before it becomes invisible. These facts may help to explain the inconsistent statements often made about the appearance of distant lights at sea. In passing, it is interesting to note that nautical regulations specify that the white mastlights of a ship should be visible at a distance of five miles, and the red and green side-lights at a distance of two miles. If it is possible to specify the range of such lights as these, it does not seem unreasonable to suggest that the utility of search-lights should also be expressed in terms of their range.

Protective Coloration.

Turning to objects which are not self-luminous, it is interesting to consider some of the factors on which the conspicuousness or otherwise of distant coloured things depends. Generally speaking, an object vanishes if its colour and reflecting power are the same as those of its surroundings.*

* This is true of objects as seen in nature, but not necessarily true for isolated objects illuminated by searchlights; for if the object is detached from its surroundings, it may "stand out" from them when illuminated by unidirectional light.

For this reason attempts have been made in selecting the colours of uniforms to match the surroundings; hence the selection of the grey-blue uniforms of the Germans and our own khaki. It is generally recognised that red is one of the most conspicuous objects at a distance, and as we shall see shortly, there are physiological reasons for this. There is no question but that modern uniforms are much less conspicuous than those of former times. There is, however, a danger of making troops *too* inconspicuous. The prolonged retention of the red trousers of the French uniform has been explained by Colonel Maude as follows. Their conspicuousness was an actual advantage to the French artillery who, in covering their troops during an advance, could readily see just how far they had progressed and send the shells the requisite distance ahead.

Chequer Work.

Another device that has been widely used in order to render objects difficult to distinguish is the use of striping or chequer. If an object—*e.g.*, a fort, aerodrome, or ship—is painted in piebald colour, its outlines are broken up, and it becomes proportionally difficult to locate. If the surrounding can also be speckled and coloured in exactly the same way it becomes almost impossible to locate from overhead. Many instances of this principle occur in nature. It is said that a zebra is more difficult to see at a distance than a horse, especially when moving, and in the East scouts have sometimes deliberately striped their horses to produce this effect. It is also said that the tiger's striped colouring is intended to render it inconspicuous among vertical reeds and grass in the jungle; while the panther, which lurks among the branches and foliage of trees, is spotted—in both cases imitating as closely as possible the surroundings, and also rendering the animal

indistinguishable owing to the "spotting effect."

One objection to the method of pigment as a means of concealment is that its utility depends on fixed surroundings. A khaki uniform, almost indistinguishable in the sand of the desert, is less so among the green fields. Even the painting of a ship grey-blue does not imitate exactly the changing surface of the sea, though it may approximate to its average hue. The Germans are said to have changed the uniforms of their scouts to white when the winter campaign in the snows of Poland began. The Turks in the Dardanelles, sniping from the foliage, are even said to have coloured their faces green!

Another possible method of concealment, more difficult to apply but more perfect in operation, would be the use of mirrored surfaces which automatically reflect the surroundings and thus cause the object to assume their hue. This method is, apparently, exemplified in the latest aluminium-coated Zeppelins, which are said to be quite difficult to distinguish in a white sky. It has occurred to me that it might be of possible application to other moving objects, such as aeroplanes, warships, and the periscopes of submarines. Conversely we have a number of suggested optical methods of making distant objects more readily visible than when viewed through the naked eye, and particularly for detecting the periscopes of submarines. I will indicate briefly a few devices of this kind, but it is perhaps better not to include them in the printed account of this lecture.

Light Signals.

Another interesting field for optical study, which likewise illustrates the application of the two principles of concealment from the enemy, combined with ready identification by friends, is signalling by means of light. Hitherto the usual practice has been to signal by flashes—alternate intervals of light and darkness. Some

protection is afforded by the use of a code, but such signals are, of their very nature, visible to many others besides those for whom they are intended. There are, however, other possible methods of conveying signals optically, and yet rendering them indistinguishable except to those who have the key.

I may next refer to several miscellaneous problems in connection with visibility.

Something more may be said as to the factors which render red objects the most distinguishable at a distance. In bright sunshine or full daylight, when the eye is adapted to strong light, vision is accomplished mainly by the central region of the retina, which is most sensitive to the red end of the spectrum. Hence, as the distance of an object increases and its retinal image falls more and more towards the centre of the eye, red objects stand out better than green ones. If two objects of equal size, one red and one blue or green, appear equally bright near by a few feet away, the red one will be found to be considerably the brighter if they are viewed at a distance. Moreover, as red is a colour which rarely occurs in nature, the colour-contrast with the background is at its maximum with red uniforms. It is a well-recognised principle among landscape gardeners that blue flowers should not be used for distant effects; it is best to mass red and orange blooms in the more remote parts of the garden.

All this applies in strong light. In a weak light, however, the central region of the retina becomes practically blind. One sees mainly by the peripheral region of the retina, which is very insensitive to red, but strongly sensitive to the blue and green rays. In twilight a bed of geraniums may become jet-black, while green grass and foliage assumes an uncanny grey hue and appears far lighter than the red flowers. It is thus conceivable that to scouts in a dark wood red coats might be an actual advantage, whereas a grey-green uniform would "stand

out." On the other hand, for advancing across a grassy field in the twilight, grey-green uniforms would be ideal.

There is another reason why green and blue objects tend to become indistinguishable at a distance, namely, the fact that the eye, not being achromatic, finds it difficult to bring these rays to a focus. An experiment devised by Shelford Bidwell took the form of projecting on a screen a slit spectrum. At close range this appeared as a sharp line, but as the observer retreats to a distance of twenty feet or so the blue end of the spectrum appears to open out like a fan. Similarly, if a bright point of light, such as a filament, is observed at a distance through cobalt glass (which permits red and blue light to pass, but suppresses the intermediate portion of the spectrum), we see a sharp red filament, surrounded by a bluish haze.

These effects are less pronounced with some eyes than others, but most people experience them. The tendency is for blue and green distant objects to become blurred and hazy, as well as losing in brightness owing to the retinal effect mentioned earlier in the lecture.

Visual Chromatism.

This want of achromatism of the eye makes itself felt in other directions. It has often been contended that the red end of the spectrum penetrates the atmosphere more easily than the blue end, and, in my opinion, this is quite true. Many natural phenomena—the ruddy appearance of the sun through a cloud or smoke, the golden hues of evening direct sunshine, and the suspended bluish haze owing to the intercepted rays of short wave length, etc.—afford support to this contention. But, since most artificial illuminants contain only a small amount of red and blue light in comparison to their total luminosity, this effect does not very greatly affect their practical utility for searchlights; moreover, in practice we could only accentuate

the red component by screening methods which suppress other rays.

The real advantage in using red and yellow light for the illumination of distant objects, lies, in my opinion, in the fact that it is advantageous to suppress the inconvenient blue rays which, so far from aiding vision, tend to produce a blurred indistinct image. We may actually see better by the loss of this light. However, the dispersion of the short wave energy throughout the atmosphere does tend to produce a luminous haze which has a certain glaring effect, which interferes with vision. This is one reason why golden reflectors have been recommended for use with searchlights and motor-car headlights. When the light is used to illuminate objects in complete darkness, the peripheral region of the retina is exceptionally sensitive to this suspended blue light, and its obscuring effect is intensified.

Advantages of Red Light.

It will be seen that all these effects favour the use of the red end of the spectrum for the illumination of distant objects. There is just one other which may act in the same way. Shelford Bidwell and other observers have remarked that in a faint light a secondary image of moving objects can sometimes be seen, following the main image and producing a certain degree of blurring. This image is most pronounced with violet light, but, so far as we know, does not occur when red rays are used.

Finally, there are one or two phenomena which are prejudicial to clear vision and deserve mention, notably the effect of glare. The inconvenient effects of a bright light in the direct field of vision, especially when one is trying to identify a feebly lighted object in almost complete darkness, are well known to you, and have been fully analysed by your chairman on several occasions. But there is another form of glare which is less well known, namely, a certain

glaring effect of even moderately brightly illuminated white surfaces, when presented to the eye with a dark background and amidst dark surroundings. In our recent investigations into the lighting of rifle-ranges we have been struck by the fact that in these circumstances it is possible to over-light a target. It would seem, therefore, that a limit should be set to the contrast between the illuminated target and the surroundings, and that the general method of shooting in a tunnel-like dark enclosed space is not altogether satisfactory.

I will now bring my lecture to a close. I am afraid its course has been somewhat disjointed, and can only plead that the material was put together at short notice, and with the aim of presenting some problems of special interest at the present time. I hope, however, that it will serve as a useful basis of discussion.

Discussion.

THE PRESIDENT said they were much indebted to Mr. Dow for a most fascinating address and one which was very *à propos* just now. He thought they all must have felt a qualm when Mr. Dow gave them a hint as to how periscopes could be made inconspicuous, because British periscopes had little chance of going out to sea against the enemy just now, whereas the reverse situation was strongly in evidence. All must have admired Mr. Dow's knowledge of physiological optics, which was as complete as it was rare. Illuminating engineers, as a rule, paid great attention to the physical side and to the mathematical side of their work, but most of them did not think that the very important physiology of vision was worthy of an equal amount of consideration. But from Mr. Dow's address it was evident that he had been a diligent student of that side of the subject, as he (the speaker) knew, because in old days (very

happy days) they used to meet and do a certain amount of experimental work together. But Mr. Dow had become so busy, and his services so much in demand, that he had had to discontinue his activities in that particular direction. He himself had no practical knowledge of searchlights, and therefore would not obtrude any platitudes on the subject, but there was one point arising out of the chromatic aberration demonstration which was of practical interest, and on which he might say a word.

When they saw that blue ring round the red centre, it was because they were myopic to the blue, and if they took a concave lens they could reduce the blue to a point, and that lens therefore would be the measure of the achromatism of the eye. Arising out of that, if they illuminated test types with red light, and the eye had no significant error of refraction, it could read the normal line in red light, but it would only read about $6/18$ ths, or a third of the way down the board, with a homogeneous blue light. Now, if they took a concave spherical lens, say $\frac{3}{4}$ D., and looked through that at the blue illuminated board, they would read the normal line, which again was a measure of the achromatism of the eye. And still further, if they took an oldish gentleman, or lady for that matter, say of 47 years of age, whose accommodative amplitude was hardly sufficient for the requirements of the interesting newspaper, they would find that the production of that myopia would obviate the necessity of advertising their meridian of life, because if they illuminated the paper with blue light, and rendered the subject of the experiment myopic to the extent of $\frac{3}{4}$ D., he could read without glasses, whereas, if the paper were illuminated with red or yellow light, he would have to use glasses.

Mr. J. Eck said Mr. Dow had been good enough to mention his name in connection with a few remarks he had made quite recently at

the Illuminating Engineering Society describing the Beck searchlight projector, the earlier stages of the development of which he had had an opportunity of seeing at the works where the first ones were constructed. The whole of the results produced had been shown on the screen by Mr. Dow, so that he had not much to say on that point, but it might be interesting to illustrate the state of perfection to which Mr. Beck had brought his searchlight by referring to some remarks which had appeared in one of the German papers, which a friend of his had seen at the Patent Office Library, in which there was an optical control of the position of the arc inside the lantern so as to retain the illuminated surface exactly in the optical focus of the parabolic mirror. This was done by means of a little selenium cell which controlled the motor which wheeled the arc mechanism backwards and forwards, and by that means the maximum efficiency of the searchlight was maintained. It seemed to him that with that little addition to the perfections of which Mr. Dow had spoken, they had really reached about the limit of possible development in searchlight projection with the electric arc. There was another use for searchlights which Mr. Dow had not mentioned, but which was quite interesting, namely, the application of searchlights to fire extinction purposes, and he recognised two of the lamps shown on the slides as being suitable for that purpose. In the great American cities, where electric light and power at a standard pressure and frequency was everywhere available, they had always a great source of power immediately at hand for setting fire pumps to work, and if we could induce ourselves in this country to adopt standard pressures and standard frequencies, we should be able to take electric pumps easily around to conflagrations, and readily apply them at proper places. And having got as far as that, we could attach searchlights to the mains and throw powerful beams

upon the burning buildings, thus enabling the firemen on the buildings to see their work more accurately, and so save many of the great losses which so frequently occurred in large cities. The question of visibility at great distances was of special interest indeed to him, and he was glad that Mr. Dow had been able to say something on the subject of the visibility of our soldiers in the field. He thought all who went about with eyes open nowadays must regret very much the service cap which had been issued to our troops, which was now, he believed, being abandoned as being such an utterly wrong device for protecting the soldiers in the field. It was wrong hygienically, but it was most wrong optically. In that cap they had a surface of the same colour as the uniform, and being at an angle reflecting the skylight, it all showed up very bright, and every cap was easily seen. Moreover, it had the additional disadvantage, as they all realised, of having a perfectly regular contour. In connection with this question of invisibility, they had probably all seen the excellent little models in the entrance to the South Kensington Natural History Museum, which showed a pair of birds, one with uniform coloration, and the other with a dark back and a light underneath part. There they at once saw the proper coloration of uniforms that should be served out to troops if they were to be rendered invisible. (Applause.)

DR. BRYAN (Royal Naval College, Greenwich) agreed with the President that Mr. Dow's lecture had been most illuminating. The first point he wanted to raise was with regard to the candle-power, a matter that had very often puzzled him owing to his ignorance of the subject. It seemed to him that if they had a point source at the focus of a parabolic mirror, they would theoretically have an infinite candle-power for the point, otherwise they would not get the parallel beam, and if the beam was divergent, then the candle-power should be the

candle-power of the image. For instance, if they put a source of light, say a square inch, in front of a lens, and then measured the candle-power of the image, say two square inches, they would find that that candle-power was twice the candle-power of the square inch of illuminating surface taken without the lens, the intrinsic brilliancy of the image being equal to that of the original source, which he believed was a theorem in ordinary optics. So that when dealing with the candle-power of searchlights there were, of course, as Mr. Dow had pointed out, great difficulties. If they were dealing with perfectly parallel pencils, then it seemed to him they were dealing with exceedingly high candle-powers, and all they had to do was to measure the absorption. With regard to that most interesting arc lamp which they had been shown on the screen—he thought it was Beck's arc lamp—he would like to ask, was the intrinsic brilliancy really increased? He thought Mr. Ritchie had mentioned that it was. Had he any measurements?

MR. RITCHIE said it was increased in the order of about five times.

DR. BRYAN said he did not mean that the whole candle-power of the lamp increased as compared with the candle-power of an ordinary lamp using the same size carbon, but was the candle-power per millimeter, or per centimeter, actually increased?

MR. RITCHIE: Yes.

DR. BRYAN: That means that the temperature is very much higher than in the case of an ordinary lamp?

MR. RITCHIE: Yes.

DR. BRYAN said he would like to have references to any actual work on that, because it was a most interesting point. Then, with regard to the blue haze which had been mentioned, he believed that some time ago a gilded mirror was brought out, which was supposed to project a yellowish beam of light, so that there was

very little blue in it, and therefore objects were rendered more visible. It was not that the light was more penetrating, but objects were rendered more clearly visible in a slight haze owing to the fact that there was very little blue light in it to be reflected back into the eye from the very small particles suspended in the atmosphere; there was no blue light to be scattered back and so cause blurring of the object seen. That had been, he believed, taken up by the Navy for some time, but he had not heard anything about it recently, and possibly it had been found to be a failure. There was one other point he might mention with regard to this blue light. A year or two ago some naval gunnery lieutenants had told him they were using blue lights to illuminate the inside of gun casemates, so that the blue light going through the apertures when the gun went off would not be seen at any distance. Now this, it was said, had had a curious effect on the gunners; it gave them a headache, and sent them to sleep, and therefore the blue light was rather unsatisfactory. He had demonstrated to them that, of course, the headache was probably due to chromatism of the eye, and suggested that they should use concave glasses to bring it into focus. The result was naturally only a smile, because one could not fit out a lot of gunners with spectacles. He had asked them about the matter again later, and had been told that they had come to the conclusion that the headache did not matter and that they could use the blue lights very satisfactorily if they kept their gunners awake. (Laughter.)

MR. CONNOLLY said he had had some slight experience in the examination of searchlights from the purely optical point of view, and though the lecturer had not mentioned it, their optical accuracy was to him really surprising. He had had an opportunity some years ago of examining one of the first commercial parabolic mirrors which it was thought might be suitable

for astronomical use, but unfortunately their optical accuracy for this special purpose was nothing like sufficient. At the present time, however, for commercial uses these mirrors fulfilled optically every requirement. Using a 24-inch mirror with a small source of light placed at a sufficient distance an image would be obtained that would vary in commercial mirrors from 3 mm. to 5 mm. in diameter. This they would realise at once was much smaller than the actual source of light in the case of a large carbon, and was, he imagined, amply small for all practical purposes. He thought it might be taken, therefore, that optically considered, as far as surfaces were concerned, success had been achieved in the case of parabolic and other mirrors used in searchlights. He would like to hear the lecturer's experience on that point, but he imagined it would be confirmatory of his own. Then there was another point. He was not at all acquainted with the system by which the increased luminosity came about in the case of the new searchlight with vapour-cooled arc. Apparently it was clear that the actual intrinsic intensity, the candle-power per square centimeter, was some five times greater, but was it five times greater for the same current intensity per square centimeter of arc? That was a question, however, simply of current consumption, and he imagined would be of little practical importance. Presumably, of course, there was five times the intensity intrinsically, but it was not clear, on the other hand, as to whether the actual area utilised was greater than in an equivalent arc of the ordinary construction, or whether the two areas were similar, the only difference being the difference in intensity. There was one other small criticism he might offer with regard to the question of polarised light in connection with the use of the periscope. Most of the light from the periscope and from the sea surface would be a very oblique reflection, and he imagined the amount of polarisa-

tion obtained would be a somewhat small percentage. He had not imagined that that would be practically very efficient, but he had not tried it and he was not prepared to say. The lecturer would perhaps give them his expert opinion on the subject. Then with respect to the achromatism of the eye, that had been mentioned as being $\frac{1}{4}$ D., and that would add effectively to the focal length of the eye only a small percentage, in which case he thought the sketch on the board was an exaggerated drawing.

THE PRESIDENT said it might vary from $\frac{1}{4}$ D. to 1.25 D.

MR. CONNOLLY said the difficulty did not seem to him to be explained merely by the consideration of the focal length.

MR. DOW thought Dr. Ettles, in dealing with the question of chromatic aberration, had answered some of the queries raised by the last speaker. His own impression was that this effect was certainly to a large extent brought about by the degree of myopia one had. He personally was a myopic, and always saw this effect very distinctly, but he had met people (Dr. Bell, of America, for instance), who declared they could focus by an effort the blue or focus the red as they liked, but could not focus both simultaneously.

THE PRESIDENT said that was no doubt quite true, because such persons were hypermetropic. A hypermetrope saw these things differently. Suppose this was the focus of the blue rays, and this the focus of the red rays, and this the retina (illustrating), by accommodation the hypermetrope could bring the first on, and by another $\frac{1}{4}$ D. of effort he could bring the other on.

MR. DOW thought that disposed of the point. Clearly the effect would appear differently to different people. Then Dr. Ettles had referred to the illumination of test types with a blue light. He had tried that himself in a very striking form by illuminating a white card on which

were traced a network of fine black lines, with a mercury vapour lamp. In front of this lamp he placed successively a deep blue glass and a red glass. When the blue glass was used, the green and yellow lines in the mercury spectrum were absolutely obliterated. One obtained pure blue light, and in these circumstances the lines on the paper entirely disappeared. On the other hand, when the red glass was in front, although there was only a very faint illumination (perhaps less than one-tenth of the other) the lines were seen quite clearly. Mr. Eck had brought out several interesting points, particularly with regard to the optical control of the position of the arc in the Beck searchlight, which was a very interesting illustration of the use of selenium, and showed the ingenuity which could be brought to bear on problems of that kind. Mr. Eck had also referred to the visibility of flat-top service caps, and he believed it was a fact that as soon as the men got to the front they pulled the wires out and made the surface as fluffy as possible. Mr. Ritchie had given some illustrations of the use of large lenses, and had given a very ingenious explanation of the effect of the high blue component in the Beck lamp. He thought that the presence of a high blue component must always be a drawback, because whether one had much or little of that light one could not focus it, but the fact that with this high blue component there might be associated a high greenish component might be a distinct advantage, because, as Mr. Ritchie had said, when the illumination was very feeble the luminosity of that green section was an important element.

LUMINOSITY AND DEFINING POWER.

Therefore one had to discriminate between luminosity and "ability to reveal detail." He remembered a series of experiments by Professor Langley many years ago, in which he adjusted the spectrum until a small object placed in the eyepiece of a spectrometer was just visi-

ble. He found the green end of the spectrum very much more effective for that purpose, and he jumped to the conclusion that the acuteness of vision reached the maximum in this region. But what he really obtained was not the part of the spectrum which gave the best definition, but the portion of the spectrum which survived longest as the light was gradually dimmed. As a matter of fact, he considered this method of extinction a dangerous one to use in photometry because of the peculiarities of the human eye introduced at such low illuminations. He had always considered it a fallacious form of instrument because, instead of working on the part of the spectrum which was the vital part in everyday life, one worked on the part which one did not care twopence for when the light was strong but which was most essential with feeble illumination; therefore, in comparing two lights one might be vastly stronger for ordinary work and yet might possess a green component which causes it to appear much the most powerful at a lower illumination. Then the question was raised as to the definition of the candle power of the searchlight. He thought that was really answered by the remarks of the subsequent speaker. He agreed if one could get an absolute "point-source," and thus get out of one's searchlight a parallel beam, one could not speak of its "candle-power" quite in the ordinary sense, because one would get the same illumination whatever distance one went away. But inasmuch as no practical source was anywhere near a point, one got not a parallel beam, but a series of diverging beams from each point of the mirror. Therefore as one went back one certainly found that the inversion square law applied fairly closely. He would like the question of the intrinsic brilliancy of the Beck arc to be dealt with by Mr. Eck and Mr. Ritchie, who were experts on that point. But he understood that the candle power indicated in the curves thrown on the screen was produced in each case for the

same consumption, so that one really did get five times the candle power for a given consumption of current. He must own it had always seemed to him a little singular that the intrinsic brilliancy of the arc should be, as it apparently was, five times that of the ordinary arc; because it had previously been assumed that the crater was at the temperature of boiling carbon. He could only imagine that if the intrinsic brilliancy was so much greater the temperature in the ordinary arc must be less than was generally supposed; presumably one could not surpass the temperature of boiling carbon. This was a point of great physical importance.

DR BRYAN, interposing, said he understood, in regard to that point that with the new arc the whole of the area was covered, whereas in the case of the other only a very small portion was. (Mr. Dow assented.) Therefore there might be five times the area with the same intrinsic brilliancy.

Mr. Dow said that this would not explain the fact that the maximum candle-power was five times greater. This candle-power was obtained by multiplying the intrinsic brilliancy by the area of the mirror. The fact of the source being greater in area would merely mean that the candle-power would fall off less rapidly on either side of the maximum. He thought, therefore, that the only effect of a larger source would be that one would get, not a higher maximum candle-power, but a greater flux of light, spread over a larger angle of dispersion.

DR. BRYAN said he was not talking of the effect of the mirror. Suppose the candle-power were normal to the face of the carbon, without any mirror at all, that would depend on the area. (Mr. Dow: Oh, certainly.) One might have five times the area of the other, with the whole face covered, and therefore the intrinsic brilliancy might not be greater, although the same current would be passing through.

Mr. Dow: Those data of Mr. Ritchie's were

obtained with the arc actually in position with the mirror. Is not that so, Mr. Ritchie?

MR. RITCHIE: Yes.

DR. BRYAN: But surely the size of the carbon in the searchlight matters?

MR. DOW: Well, it matters from a practical standpoint, but from the standpoint of producing a maximum candle-power in a certain direction there is no advantage in increasing the size of the crater.

DR. BRYAN: I cannot quite understand that.

MR. DOW went on to say that reference had been made to the use of mirrors of a gilded character. He believed that that mirror, although not much had been said about it, was being developed and used, and that the advantages claimed for it had a certain amount of justification. With reference to the blue light sending people to sleep, that was one of those physiological points which were always cropping up, and always being explained, and yet never getting verified. There were books in America on the therapeutic effects of light, full of descriptions of such phenomena; it was said that dentists drew out teeth by means of a blue light, and it was even suggested that by the use of a strongly focused blue light it was possible to obtain local anæsthesia. He had asked Dr. Ettles once whether he had ever tried to carry out an operation by that method, but his reply was, "Nothing doing." (Laughter.) It was suggested that in some barbarous countries revolutionists were put into prisons roofed with blue glass, and after a time the blue light so tamed them down that they were able to be let out in a perfectly harmless condition. (Renewed laughter.) All those statements seemed to require a certain amount of scientific verification. He quite appreciated what had been said by Mr. Connolly as to the perfection of the modern searchlight mirror, and did not think anything much more could be done as far as optical accuracy was concerned. Such variations from the

theoretical as occurred were attributable rather to imperfections in the light source. With regard to the question of the effect of polarised light in connection with the periscope, the idea was not that the light from the periscope was polarised, but the light from the sea, hence the use of an analyser might accentuate the contrast between them. Instruments for use at sea were sometimes provided with analysing apparatus to make the surface of sea look dark, the analyser being set to quench the light polarised by reflection. The idea was that the brightness of the periscope would not be altered, but the surrounding region of the sea would be. (Applause.)

THE ACHROMATISM OF SEPARATED LENSES.

BY MR. T. B. VINTYCOMB.

Read 3rd June, 1915.

THEORY shows that it is impossible to make a pair of single lenses achromatic at the same time for focal length and for the position of the principal focus. Frequently achromatism for focal length is sufficient, and then it is shown that two lenses, of glasses whose dispersive powers are ω_1, ω_2 , form an achromatic combination when the distance apart d is given by

$$\frac{\omega_1}{f_1 - d} = \frac{\omega_2}{d - f_2}.$$

When the two lenses are of the same glass this gives

$$d = \frac{f_1 + f_2}{2}.$$

It seemed to me worth while to enquire what were the conditions under which this achromatism would hold for an extended range of the spectrum. When $d=0$ it is well known that the condition is that the ratios of the partial dispersions to the mean dispersion should be equal for the two varieties of glass.

This is of more interest as all the text-books on optics state that when the two separated lenses are of the same glass, the secondary spectrum is completely eliminated from the focal length. This is quite untrue, and is

caused by a misinterpretation of the formulæ. On the other hand, in this case the variation of the focal length is small, and it is proportional to the square of the dispersive power.

Writing

$$\psi = \frac{1}{f} = (n - 1) \left[\frac{1}{r} + \frac{1}{s} \right] = m K$$

$$m = n - 1, \quad K = \frac{1}{r} + \frac{1}{s}$$

the power of two separated lenses is

$$\psi = m_1 K_1 + m_2 K_2 - m_1 m_2 K_1 K_2 d.$$

If $m_1 = m_2$

$$\psi = m (K_1 + K_2) - m^2 K_1 K_2 d$$

a parabola for ψ in terms of m .

We see $\psi = 0$ if $m = 0$

$$\text{or } m = \frac{K_1 + K_2}{K_1 K_2 d}.$$

$$\text{Then } d = \frac{m (K_1 + K_2)}{m^2 K_1 K_2} = f_1 + f_2$$

$\psi = \psi_0$ is max. for

$$m_0 = \frac{K_1 + K_2}{2 K_1 K_2 d} \quad \text{or} \quad d = \frac{f_1 + f_2}{2}$$

$$\psi_0 = m_0^2 K_1 K_2 d$$

$$\psi = (2 m m_0 - m^2) K_1 K_2 d$$

$$\psi_0 - \psi = (m_0 - m)^2 K_1 K_2 d$$

$$1 - \frac{\psi}{\psi_0} = \left[\frac{m_0 - m}{m_0} \right]^2$$

This gives the same variation of focal length with colour for all ratios of f_1 and f_2 to d

Can better achromatism be obtained using different glasses for the two lenses?

$$\psi = m_1 K_1 + m_2 K_2 - m_1 m_2 K_1 K_2 d$$

$$\frac{\delta\psi}{\delta\lambda} = A_1 K_1 + A_2 K_2 - A_1 m_2 K_1 K_2 d - A_2 m_1 K_1 K_2 d$$

$$\frac{\delta^2\psi}{\delta\lambda^2} = B_1 K_1 + B_2 K_2 - B_1 m_2 K_1 K_2 d - B_2 m_1 K_1 K_2 d - 2 A_1 A_2 K_1 K_2 d$$

writing

$$A = \frac{\delta m}{\delta\lambda}, \quad B = \frac{\delta^2 m}{\delta\lambda^2}$$

where λ may be any parameter which defines the particular colour. I have used the wave-length,

$$\frac{\delta\psi}{\delta\lambda} = 0 \text{ gives}$$

$$\frac{A_1 K_1}{1 - m_1 K_1 d} = \frac{A_2 K_2}{m_2 K_2 d - 1} = h \text{ say (i.)}$$

$$\text{Now } \omega = \frac{A}{m} \delta\lambda$$

$$\text{so } h \delta\lambda = \frac{\omega_1}{f_1 - d} = \frac{\omega_2}{d - f_2}$$

the usual formula.

$$\frac{\delta^2\psi}{\delta\lambda^2} = 0 \text{ gives}$$

$$B_1 K_1 (1 - m_2 K_2 d) + B_2 K_2 (1 - m_1 K_1 d) = 2 A_1 A_2 K_1 K_2 d$$

combining this with (i.) gives

$$\frac{B_1}{A_1} - \frac{B_2}{A_2} = -2 h d \dots\dots\dots \text{(ii.)}$$

If 1 and 2 both hold the curve giving ψ to λ is horizontal, and has a point of inflexion for a particular value of λ .

By slight changes in the values assumed for

$$\frac{A_1}{m_1}, \frac{A_2}{m_2} \text{ and } X = \frac{B_1}{A_1} - \frac{B_2}{A_2}$$

we may be able to modify the form of the curve and improve the achromatism by reducing the salient parts of the curve.

Calculations.

To find the range of applicability of the formulæ, I calculated first for several varieties of optical glass the values of A, B, etc. This was done graphically from the values of the index for selected spectral lines.

The results are given on next page.

The accuracy of these figures I estimate at about 1 per cent. At least, I claim no higher accuracy.

This table gives an idea of the range of values met with. The unit taken for λ was 1 micron,

$$\text{so } \omega \times .17 = \frac{A}{m} \text{ approx. as } \lambda_c - \lambda_v = .170$$

O .103 is a dense flint $v = 36.2$. O .60 a hard crown $v = 60.2$, both of the older types of glass.

Then I calculated the values of $f_1 f_2$ and f for a pair of glasses of these dispersive powers, assuming a range of values for

$$X = \frac{B_1}{A_1} - \frac{B_2}{A_2}$$

from .01 to 1.6.

The results are given in diagrams 1 and 2.

Glass.	No.	A.	B.	A m	B		Jena glasses. Name of glass assigned from index, etc.
					A	B	
O. 165	... 1.7513	185	76	169	... 5.62	...	Jena glasses. From catalogue.
S. 386	... 1.8819	1975	... 1.20	222	... 6.07	...	
O. 103	... 1.6202	0875	462	141	... 5.28	...	Jena glasses. From catalogue.
S. 7	... 1.6086	0700	890	115	... 5.57	...	
S. 8	... 1.5786	0584	279	102	... 4.78	...	Jena glasses. From catalogue.
O. 60	... 1.5179	0483	215	085	... 4.86	...	
S. 30	... 1.5760	0455	305	079	... 4.51	...	

The formulæ used were derived from 1 and 2, and are simple.

$$X = -2hd$$

$$\begin{aligned} & -2d \frac{\Lambda_1}{m_1} \\ &= \frac{f_1 - d}{d - f_2} = -2d \frac{\Lambda_2}{m_2} \end{aligned}$$

giving

$$\frac{f_1}{d} = 1 - \frac{2\Lambda_1}{X}$$

$$\frac{f_2}{d} = 1 + \frac{2\Lambda_2}{X}$$

$$\frac{f}{d} = \frac{\frac{f_1 f_2}{d d}}{\frac{f_1}{d} + \frac{f_2}{d} - 1}$$

As the diagram 1 shows, the cases may be separated into two classes.

1. f long compared to d —O G type. These are only useful when X is extremely small, as otherwise the curvatures of the individual lenses become too great.
2. f nearly equal to d —E P type.

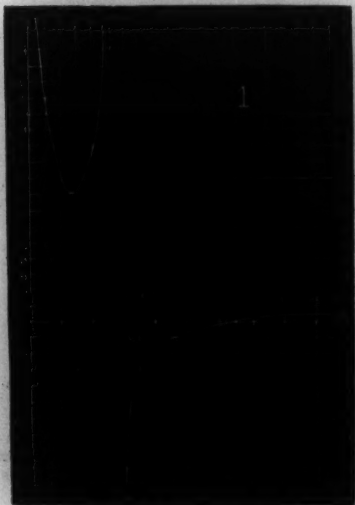
These two classes are separated by a peculiar case; a telescope free from secondary spectrum composed of two simple lenses. The magnifying

power is small, being the ratio of the dispersive powers.

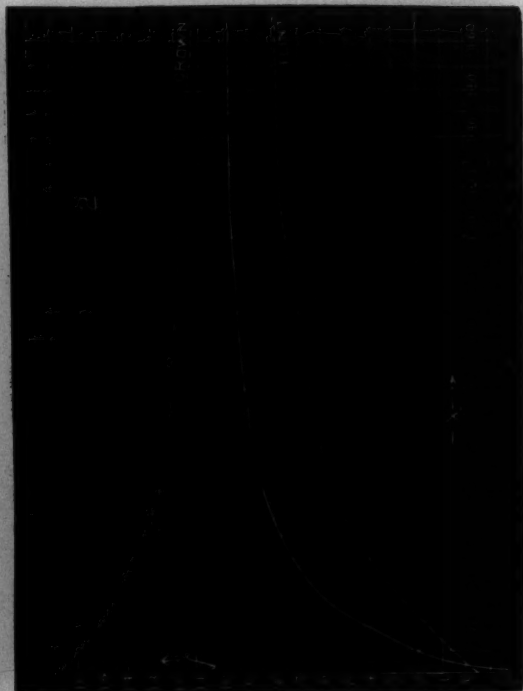
For when $d = f_1 + f_2$

$$\text{and } \frac{\omega_1}{f_1 - d} = \frac{\omega_2}{d - f_2}$$

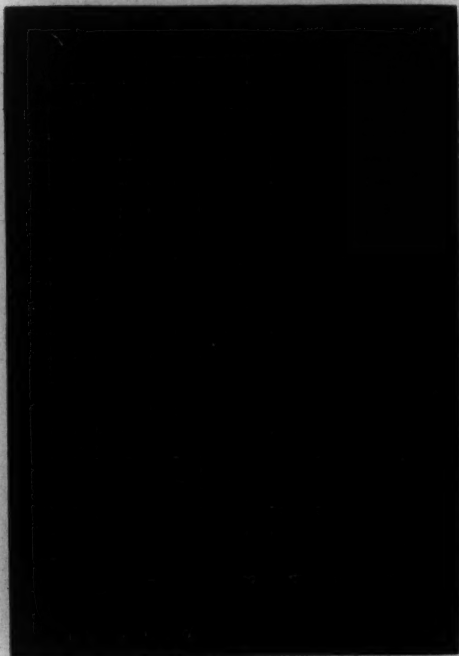
$$\text{we have } \frac{f_1}{f_2} = - \frac{\omega_2}{\omega_1}.$$



We can see that in Class 1 a slight separation of the lenses might improve the remnants of the secondary spectrum, but the partial dispersions must be *very* nearly proportional for a useful combination.



In Class 2, as X increases, f_1 is at first $-$ and afterwards becomes $+$. The Ramsden type is



approached as a limit for large values of X . The flint lens has always a shorter focal length than the crown, and approaches d as an upper

limit. d is the lower limit of the focal length of the crown lens.

I worked out an actual case for the glasses O .103 and O .60, as it seemed that the results obtained with a pair of known type would show what was possible in this way, better than if I had chosen a pair of special glasses.

The arrangement given directly by the formulae, from the calculated values of the constants, had a horizontal point of inflexion as expected. There was, however, a pronounced salient for shorter wave-length, and a rapid drop of ψ for larger wave-lengths. By modifying the values of the constants slightly in turn the form of the curve giving ψ in terms of λ can be much improved, so that the variation in ψ is less than $\frac{1}{10000}$ over the most of the visible spectrum. This is given by the full line in diag. 3, marked VIII.

For comparison I give the curves of ψ for an achromatic pair of glass O .60 and for a pair of glass O .103. These show the advantage of using a glass of low dispersive power in eyepieces of this type, and the very good achromatism attained in using two lenses of the same glass.

COMBINATION No. VIII.

$$\frac{f_1}{d} = .610, \quad \frac{A_1}{m} = .156, \quad \frac{A_2}{m} = .097$$

$$\frac{f_2}{d} = 1.2425 \quad X = .8$$

$$\frac{f}{d} = 1.12$$

$$\frac{\psi_0 - \psi}{\psi_0},$$

A'	C	D	F	G'
-.000066	-.000087	0	-.000025	-.000495

Discussion.

MR. T. F. CONNOLLY said he had listened to Mr. Vinycomb's paper with considerable interest, but he was not quite sure as to its practical value. Mr. Vinycomb had shown quite clearly that the ordinary formula for the focal length was defective if the same glass was used for both lenses, and by his manipulation he had shown them how he had achieved a flattened curve. First of all he got the first differential co-efficient, then the second, then by a careful selection he caused the point of inflection to be more or less parallel to his axis. But there was very little variation in the power, and the variation in wave length was also small. Practically all the curves he had shown them would indicate a very small variation even in extreme cases, and while the point was quite conclusively proved and the demonstration was of some beauty, he did not know that it was of practical importance.

MR. VINYCOMB said he had not had much experience of the practical side of optics otherwise than having to teach the subject. The achromatism of eyepieces was very good in the ordinary course, and as a matter of fact he believed it was ample for what was required, as the achromatism was not the main trouble in an eyepiece, or anything like it. Still, it had been his wish to see how far it was capable of improvement, and what were the consequences of the formula, and also he did not think it was a good thing to have a mistake in the books. But he did not think that his researches were really likely to cause any change in the practice, as it only applied to the eyepiece type really.

MR. CONNOLLY thought perhaps Mr. Vinycomb was hardly fair to the books. In his opinion it would be truer to say that many books were limited in their statements than that they misstated the case.

MR. VINYCOMB referred to Heath's, Herman's, and Drude's books, all of which, he said, defin-

itely stated the matter wrongly. Denis Taylor did not say a word about it, because he did not make statements he was not able to cover; but the other books went out of their way to do so. That had been the main cause of his interest in the subject, and he had thought it might possibly be of interest to some members of the Society to have the thing put straight.

OPTICAL ACCESSORIES.

BY MR. WILLIAM SALT.

Read 3rd June, 1916.

SINCE I rather thoughtlessly consented some time ago to prepare a paper on "Optical Accessories," I have arrived at the conclusion that to deal with so wide a subject in anything like a comprehensive manner would require a great deal more time than the usual duration of a meeting of this kind. Indeed, the subject is so very wide in scope that it might very well be divided into several parts, each of sufficient importance to warrant especial attention as a thing by itself. Therefore it does not appear to me an easy task to grapple with the subject in what I would regard as an intelligent manner unless I approach it in a specific rather than in a general way. With your indulgence, this is what I propose to do for, if we consider what things may reasonably be classed as optical accessories, we shall soon discover that a very long list confronts us.

I do not know who is responsible for the title of the present paper—it certainly is not myself—but whoever it may be, I will assume that the idea was to bring out some thoughts which might be at least of ordinary interest, particularly to those members of the Optical Society who devote themselves principally to the work of dealing with the correction of visual error rather than to optical work in a wider sense. If I am not mistaken, a very large proportion of the present members of the Society may be taken to come within this classification; and also, if I am not again mistaken, it may be

assumed that the motive of this paper will be mainly, although not entirely perhaps, to refer to the numerous instruments and things with which the spectacle optician has, or might well have, intimate concern; and this in the commercial sense principally.

Let us briefly consider what is the popular understanding in these times of what an optician is. Probably in some instances the man in the street regards the optician simply as a person whose work it is to provide him relief at what time he discovers that he can no longer see familiar objects so well as formerly—that is, a maker and vender of spectacles and the like things. This, however, should not be taken to imply ignorance on the part of the public, but rather it presumes nothing worse than absence of reflection, which in these days of rush and necessary rapidity of action and thought is excusable. But in by far the majority of instances the popular conception of the optician is that he is an individual who is skilled in knowledge and production not only of lenses themselves, but also of all mechanical contrivances that are requisite in connection with the employment of the various kinds of lenses, as well as of the numerous subsidiary things designed for the convenience of wearers of spectacles and users of all other optical instruments.

Otherwise than in very exceptional cases, it is not to be expected that there can be found in the establishment of an optician at all times every object capable of being counted an optical instrument or appliance. Nor can it be thought seriously that any one individual is capable of possessing both scientific and practical knowledge of the creation and application of all such instruments and appliances in anything like complete degree. It is inevitable that there must be division of labour, so to speak—a separation of the whole into more or less distinct departments—if confusion is to be avoided and justice

done in every direction. Happily this is the tendency in recent times. We are finding to-day that the microscope maker as a rule confines his energies almost exclusively to that class of work; the maker of telescopes is not much disposed to seek for work involving at any rate a wide departure from his particular art; the lens worker finds enough to occupy all his attention in the making of lenses; the manufacturer of photographic cameras as a rule is disinclined to turn his attention in any direction other than that of the production of cameras; the spectacle maker nowadays desires little else than to be regarded as an expert maker of spectacles, and does not seek a much wider field. And so one may go on through the numerous departments of the wide field of the optical industry. This is, no doubt, also the case in most other industries. It is becoming more and more a recognised rule that specialisation is advantageous all round.

All this, however, relates particularly to the actual manufacturer or producer of an article rather than to the mere seller or dealer in the ordinary commercial sense. It is rather more to my present purpose to make reference to what may be appropriate or convenient to the trader as distinct from the producer. We all know that while there are abundant instances where the manufacturer himself is also the one who sells direct to the individual user the articles he produces, in the majority of cases the manufacturer desires his productions to reach the users thereof through the retail trader. As a general rule this is not only the common custom, but also it is a convenient way of providing channels for circulation of the products—convenient alike to the manufacturer, the trader, and the ultimate purchaser, as well as to the benefit of each. Indeed, in some instances, at all events, the procedure is for the productions of a manufacturer to be handled by more than one intermediary before they reach the actual user.

Such intermediate dealing carries with it im-

portant advantages not by any means confined to the intermediary himself. In many cases—perhaps in most—the manufacturer for one reason or another finds it awkward, if not impossible, to arrange by his own efforts that his productions shall come reasonably under the notice of those who are particularly interested in them, while at the same time he very naturally desires publicity for his work. On the other hand the individual desirous of purchasing a commodity finds it to his benefit to be able to call at some place to inspect the article, and, if need be, to have its functions explained or its qualities discussed, before he makes his purchase; which, of course, could not be done, except in relatively very few instances, otherwise than by the aid of the intervening dealer suitably familiar with knowledge of whatever article is in question. In this way the intermediary is indispensable, and my present remarks apply principally to him. He receives his reward in the profit he gains by selling the goods in his hands; the manufacturer has his advantage, too; the user of the article obtains his share of satisfaction; and each enjoys benefit from the actions of the others. Of course, all this can be said in relation to each department of the optical trade, and therefore it applies to that branch which I will refer to as that of the spectacle optician.

The Sight-Testing Optician.

Now, the spectacle optician, or refractionist, optologist, optometrist—well, by whatever name he may be called—is a trader without doubt, but one whose vocation is entitled to be classed almost as a scientific profession, thanks largely to the elevating influence exercised by the examining bodies happily existent, and as such, his work lies, or should lie, in a much higher plane than that of the ordinary buyer and seller of mere commonplace articles. But although this may be said of him, it would be a

grave mistake for him to forget that he is a trader, or to allow himself to be deluded by the idea that, because his calling necessitates a considerable amount of studious work, he is not a trader. If this be conceded, it can also be accepted that our spectacle optician loses no dignity whatever in having for sale in his establishment all or any of the numerous things required or desired by those who have cause to employ the use of lenses to rectify their visual defects. And the optician is none the less an optician if he sells, as well as his spectacles and other optical goods, also the numerous other things which can very rightly be called accessories to his business. Moreover, the selling of such other things contributes not only to his own profit but also to the convenience of those who come to him for his lenses.

Scope of the Trade.

I know of one or two instances where the spectacle optician assumes it to be beneath his dignity to offer, or expose in his establishment for sale, any article whatsoever besides the actual lenses and their accompanying necessary mountings, which he prepares and fits in painstaking manner to the requirements of those who visit him. Yet he is a trader all the same, because he sells for profit those very lenses and mountings. His customer, maybe, asks for this, that, or another thing frequently or commonly required by the wearer of spectacles, and is told by our worthy optician that he does not keep them. What is likely to be the result? However attractive our optician may be, we will say, by reason of his personality, his dexterous skill, his learning, or what not, it cannot be denied that his disinclination to be prepared to furnish any one of the articles legitimately accessory to his business drives the customer to some rival establishment where he may find, not only the thing he is at present in search

of, but also perhaps inducement to patronise the other optician in the future.

In this connection I am tempted to interpose a suggestion. The optician I have just referred to is he who esteems himself competent to decide, after examination by the usual methods, precisely what, if any, correction by lenses is required to restore the normal standard of vision. In a word, the sight-testing optician with exalted ideas, who deems it expedient to fix on the mind of his subjects an impression of importance superior to that of a trader. In such a case it could be made to accord with the optician's sensitiveness, and yet yield him financial gain, if he himself would act only so far as to determine what lenses are required and then refer the subject to an adjoining room in his establishment, where considerations of quality of lenses or kind of mountings, cases, and other adjuncts, could be discussed and commercially dealt with by a subordinate.

But I think that cases such as this are very few. On the other hand we all know of instances where almost the extreme reverse description applies—where the optician allows his establishment to be burdened with things that have very little relation to optics, if any at all. However, such instances are not now as frequently found as in former years.

Shop-worn Goods.

I remember well the time when, judging by what was to be found on the premises of many an optician, it seemed to be the rule for the vender of spectacles and other optical instruments to include in his stock many articles that were no more related to optics than—may I say—the Optical Society is related to a place known as Billingsgate, where, as I am led to understand, the supply of fish for human food is the principal occupation of the estimable gentlemen who operate there. I do not forget how it seemed to be that the window of the optician.

in the far-off days to which I refer, represented that the owner thereof was desirous of alluring the public to his place to purchase, first of all, not optical things, but one or more of a seemingly endless variety of model locomotives and stationary engines, worked by steam power; secondly, some mechanical toy—ingenious enough, I admit—of the jack-in-the-box order; a magnetic compass from a collection of specimens of such things that no doubt at some time had been bright and fresh-looking; or a gyroscope perhaps, furnished with rings of paper of various colours, which, when set in motion, so greatly delighted the youngsters at home if it did not as well, and perhaps to greater extent, contribute to the delectation of their elders.

Then, to follow up the sequence according to the importance of things as represented by the strength of the display, the window invited the purchase of a quadrant or a sextant out of several that at some time or other had doubtless looked at their best; or of a box of drawing pins, a case of drawing instruments, a chronometer, an ancient-looking timepiece, guaranteed to be in good order, if the legend upon it was to be believed. Besides these things there were to be seen other objects prominently displayed, many of them without even a semblance of optical work about them. Lastly, as it seemed to me, by reason of the relatively inconspicuous position they occupied amongst the general display, attention was invited to a more or less attractive assortment of optical goods, starting perhaps with a number of spectacle frames containing lenses and protected by the legend, "To suit all sights." These articles were usually to be seen spread or otherwise disposed upon a tray or two in a manner so bewilderingly confused as to convey the idea that the proprietor was so constantly occupied with that branch of his calling as to leave him no time to provide that the display should have the amount of attention it deserved.

Confusion in Window Displays.

As a rule, there was a raised platform or gallery occupied by a row or two of Galilean binoculars of various sorts and sizes, some without any particular mark to entice the observer, except maybe an attached card intimating the price one would have to pay to become possessed of a specimen, and others bearing cards upon which were inscriptions proclaiming the ability of the instruments to "carry" I do not remember how many miles. I could never make out why it was that such things as these were usually placed upon a raised platform, generally in the background. Maybe it was due to a desire to demonstrate their power to "carry" a long distance. Nor could I help noticing with respect to this raised platform that the commendable thoughtfulness of the proprietor of the establishment, in providing that periodical rubbing up of the articles upon it should be performed, had the effect—perhaps coupled with the action of sunshine through many days—of rather considerably altering the original colour and appearance of the articles it supported. But who cared much for appearances in those days? Here and there, at all events, if not in the windows of most opticians of the times I refer to, could be seen a telescope or two of large dimensions, imposing enough to set a galloping pace to the thoughtful mind. And microscopes, too, in rather larger number. It might have been the result of accidental circumstance, or it might have been due to a thoughtful desire to shield these valuable instruments from the sinister effect of a blast of cold wind, but I do know that they were usually so very closely surrounded by a conglomeration of the various sorts of mechanical models, and other miscellaneous non-optical things to which I have already referred, that one might well have been excused if at the first, or even the second, glance one failed to realise their existence there.

Before going further I would like to make it

quite clear that it is not my intention or wish to deride those who in fact laid the foundation of the optical industry as it is found to-day. My object is, if possible, to "point a moral and adorn a tale," and with all modesty I think that to compare the past with the present may serve some useful purpose to us to-day, even if that service may be only of small degree. The worthy optician of the years gone by doubtless knew what most appealed to the public desire and taste in those times, and we of the present day ought to assume that he did the best that could then be done for the service of those who had need or desire to seek his assistance. If in his judgment he decided that a widely diverse collection of articles such as I have mentioned could be considered legitimate accessories to the calling of an optician, well and good, but we of these later times will assuredly take a different view.

Varieties of Trade.

But let us hasten to state that my description of an optician of past days does not necessarily apply to all of them in those times. Indeed, it ought to be pronounced that there were some who made optical work their sole concern, and happily it can be stated that a number of these continue to exist to-day with high renown attaching to their names. The description, however, honestly applied to a good number, not only whose *locale* was of small importance in regard to numerical value of the immediately surrounding population, but equally to some who practised their vocation in populous towns or districts, if not as well to others in the metropolis itself.

In reviewing what was in the past, I think that most of us to-day will conclude that some of the methods of the optician of former days can hardly be regarded as appropriate at the present time. Nevertheless, we must not forget that there are many instances where an optician

finds himself obliged by the force of circumstances, and in order to assure a livelihood, to rely to some extent upon dealing in things apart from strictly optical contrivances. Such instances arise from one cause or another, but that they actually exist very few will doubt. It is not the lot of every optician of sound repute to be so placed for it to be possible for him to depend entirely upon his activities in relation to purely optical matters. Due to particular circumstances, it may be, and oftentimes it is the case that an individual well versed in optical science and all the work it entails, finds himself established in a locality where the optical requirements of the population, while enough to call for serious attention, are insufficient in themselves to bring to him the pecuniary support to which he is entitled.

What can one in such a position do? Obviously, he must seek some further means of obtaining what he lacks. But he is first of all an optician, and above all else his concern is in optical work. For him that work should stand paramount. But if he decides to devote some part of his attention to other than strictly optical things, he ought at least to take heed that the additional articles he sells are such as will occupy as nearly as possible as high a place in public estimation as optical instruments themselves.

Let us take the case of the optician who has aimed at spectacle work alone, and found that it brought him but scanty recompense. He naturally desires to augment his earning power, and to this end he decides to traffic in other articles as well. He may well do this without going far astray from the legitimate bounds of his original work. His first step should be to assure himself that he has given sufficient consideration to the many appliances which especially pertain to spectacles, otherwise he would not be just to himself nor loyal to his calling.

Without the slightest loss of dignity our spectacle optician could let the things he has for sale include many articles which, although not strictly optical in themselves, are at all events capable of contributing to the convenience of wearers of spectacles, and such things may therefore be looked upon as adjuncts that can rightly be classed as distinctly accessory to his business.

Eye Protectors.

Without mentioning the very obvious wisdom of being prepared to exhibit as wide a choice as possible of styles and qualities of frames and other mountings for spectacle lenses, reference may well be made in this connection to the potent usefulness of letting one's stock include an assortment, as attractive as possible, of such things, for example, as coloured glasses, and the very many sorts of goggles and contrivances required by thousands who seek to find a means of protecting their eyes from inconvenient glare, or wind, or dust, or what not. If observation (and indeed report also) does not betray me, the optician—perhaps by reason of nothing more than thoughtless indifference—has to a great extent allowed the exploitation of such things to pass from his hands into the infinitely less appropriate or capable hands of traders of quite a different class. This is a deplorable fact and worthy of serious attention. Surely it is a truism that, if the oculist is not consulted, the optician only should be appealed to when the welfare of the human eye is in question.

Furthermore, it is decidedly appropriate that good attention should be given to the numerous useful things contrived for the purpose of preventing loss or misplacement of "aids to vision," or of accidental damage thereto—such articles as cords, chains, and hooks, for securing the glasses about the wearer's person, and quite a number of other things too, which, are

to be procured in so great a variety as to satisfy the desires of everyone from the most severely modest right along to the extremely ostentatious. All such things are so closely related to the wares our spectacle optician primarily deals in that they can well be classed as very essential to his business. And although to handle such things requires neither particular training nor anything approaching profound scientific knowledge, it does not, even in the smallest degree, entail sacrifice of the dignity which the optician is, or should be, entitled to enjoy. At the same time it results in pecuniary profit, and that, of course, is what every trader seeks to obtain.

Cases.

Again, the subject of appropriate containing receptacles, for spectacles and the like, deserves very serious consideration, as well for the advantage of the dealer as for the satisfaction of his customer. But here I will venture upon a digression of a somewhat personal nature. Lest otherwise my motive should be misunderstood, I desire to assert that, although, as some of you may be aware, I have the advantage of possessing at least a superficial acquaintance with such commodities as cases, I refer to them now only and solely because such things *must* be regarded as essentially accessory to the business of the optician.

Notwithstanding that amongst those interested it is common knowledge that divers manufacturers take pains to produce cases in immense variety of form, of size, of quality, and of attractiveness, some opticians appear to be content to believe that purchasers of spectacle work—high class or low class, or anything intermediate—are satisfied with almost the simplest and plainest contrivance capable of protecting a pair of lenses from early destruction. This is an unfortunate belief, for in the first place it is erroneous, and again because it may tend to create a feeling of dissatisfaction in

the mind of the purchaser. Some may assume that so long as the case supplied with the glasses protects them from harm when they are not actually in wear, its intrinsic value does not matter. Others may think that, as they do not make a charge for the cases they furnish to their customers for glasses, they cannot afford to supply a thing that costs them more than a very few pence. Both assumptions are exposed to very severe objection. The first does not succeed because it is directly opposed to public desire and preference.

The Inclusive Price.

Nowadays the public taste is accustomed to artistic refinement, and hankers for something decidedly superior to the mere commonplace. This is so in relation to articles of far less consequence than such important things as contrivances intended to correct defects of human vision, and therefore it must be quite as much so in relation to such contrivances. The second assumption also fails, for the reason that, although the optician does not ordinarily make a *separate* charge for the case he delivers with a pair of spectacles, he none the less receives pay for it, because in calculating the price for which his spectacles may be purchased, so as to leave him a profit for his work, he has already taken into account not only the cost to him of the spectacles themselves—the frame, the lenses, the workmanship, the service, it may be—but also the cost of the case itself. Surely this cannot be disputed. And if it be granted, it would as surely be a simple thing for our optician to let his total estimation of cost include the value of a case whose excellence is in keeping with that of the spectacles it is intended to contain. It may be argued that to do this would make larger the price the purchaser would have to pay. That may be so, but hardly to such an extent as to cause dissatisfaction to the purchaser. On the contrary, it is more than likely that it would increase the satisfaction which the

carefully adapted spectacles themselves might rightly be expected to give. Just as a person finds pleasure in being well dressed, or in having any of his belongings well appointed, so would he enjoy gratification in seeing his glasses as pleasingly encased.

There is another aspect, however. Any optician may well gain advantage by letting it be seen that he has for sale these, or any other articles pertaining to his calling, made of more expensive materials than those which usually prevail, and fashioned in more elaborate manner. There always were, and no doubt always will be, persons whose æsthetic tastes or other leanings lead them to the desire to possess whatever pleases their fancy, and, in so far as their pockets will admit, they do not rest content until that desire is satisfied. Many a sale could be encouraged by the exhibition of articles likely to interest such persons. Herein is opportunity for our optician to increase the volume of his business without going away from his legitimate sphere, and at the same time to attract to his place time and again those whom he has delighted not only with the excellence of his spectacle work, but also with things subsidiary thereto.

The Finished Optician.

Let us see if we cannot travel a little wider still, but before doing this we may as well pause to take a side glance at things, even if by doing so we do not discover a lesson. I make no excuse for continuing to harp on the spectacle optician, because he stands for a rather new order of things in the optical world, and as such deserves much notice. He is now to be found in every city and town throughout the length and breadth of our islands, and he is becoming more and more of importance in the public estimation. But let him beware lest this very fact should lead him to forget what he really is. Alas! I have heard of some instances where a young man, after having studi-

ously worked for and obtained, with conspicuous success perhaps, a certificate of proficiency in optical work, has apparently allowed that very success to overcome his commercial judgment. To have passed the optical examinations—severe as they may be—even triumphantly, is not an indication that the one who has so succeeded is a scientific genius. In almost every case he has braved the ordeal with the intention of becoming an optician. Having successfully passed through the test, he is entitled to commence to learn what it is to be a sound optician, but by no means ought he to conclude that, because he has safely emerged from the fire, he is the "finished article," and that his word on optical subjects is the law.

The man who becomes an optician and is so indifferent to the commercial side of his calling as to forget that he is none other than a trader, is doomed to grave disappointment. The one who brings himself to imagine that, because he has obtained success in the examination, he is a perfect optician, has turned aside from the path leading to the goal of his desire even at the moment he has set foot upon it. However, the object of this brief side-glance is not to point to blame, but to indicate a warning, and to suggest that an accessory of the highest importance to the business of an optician, and as sure as any other a guide to ultimate success, is the determination of the optician himself to believe, and to always act upon the belief, that his work and his achievements day by day are but steps upon the way towards perfection, which however can never be completely attained.

Optical and Meteorological Instruments.

If our optician still finds that he has not enough with spectacle work and with the various things clearly pertaining to spectacles added, let him include as well one of the other branches of the optical industry. But let him, for his own sake, take good care to maintain the dig-

nity which should be his, and not risk the loss of it by mixing with optical things anything calculated to remind one of the toy shop. Telescopes, including binocular glasses of course, seem to be the natural next-door neighbours of spectacles, and, unlike such branches as that covering photographic goods, or that confined to microscopes, or that branch having to do with magic lanterns and projecting apparatus in general, all of which take in almost an infinite variety of things, they are easily handled, occupy but little space, and a useful assortment of them does not necessarily involve a large expenditure of capital. When selling a pair of spectacles it is often but a short step to take to interest a customer to purchase a telescope or a binocular glass. Furthermore, such things as barometers and thermometers, although not strictly optical, could suitably be added to the stock without obscuring the main chance, and indeed would usefully assist it. Such things are in frequent demand, and the public have become so accustomed to look to the optician for them that they may with reason be looked upon as accessory to the optician's business.

All who are here know very well that the wide range of instruments falling under the titles "meteorological," "philosophical," "mathematical," "surveying," "nautical," and "photographic" are usually somewhat closely associated with the optician's business. But how impossible it would be for any one establishment to do justice to all these as well as optical goods!—unless, indeed, it were of immense size and divided into a number of distinct departments, each with a suitably large and competent staff attached to it. And none need be told that such instruments as the ophthalmoscope, the ophthalmometer, and its twin brother the keratometer, the retinoscope, and other such things as are used—or at all events ostensibly employed—by hundreds of

opticians who, with more or less authentic reason, claim to be experts in ophthalmology, are optical accessories, but of a sort that could more appropriately be considered were the title of this paper, say, ophthalmological instruments. Moreover, each in itself is, as we know very well, of sufficient importance to deserve a separate treatise.

The trial case along with its ancillary trial frame is most expressly an optical accessory, without which no spectacle optician can do his work. But here again is a subject large enough for separate treatment. It is my pleasure to find that the trial frame itself is being dealt with to-night by one who is extremely capable of demonstrating its full purposes.

Resources of Scientific Research.

Although these notes may appear to dwell persistently upon the frigid aspect of the commercial side of the optician's business, it is not intended that the more charming view should be overlooked. In the sphere of optical work the commercial consideration is not the only one of importance. We have as well to think of the renown which is the reward of painstaking effort and continuous striving to apply with good effect the resources of scientific research and mechanical contrivance to the benefit of humanity when natural conditions have become disordered. Recognition of this consideration, and the acquisition of the renown now indicated, are also in most gratifying sense accessories to the work of the optician.

Now let us indulge in one further thought. The whole field of optical work is far too wide to be completely encircled in proper manner by the efforts of any single individual, or even of a group of individuals comprised in one establishment; therefore the need for specialisation is manifest. We all know what is meant when the shoemaker is admonished to "stick to his

last." So should the spectacle maker confine himself as closely as possible to his work, including everything that rightly pertains to it. Likewise the producer or seller of microscopes, of telescopes, of photographic, and other things. And the same should be with every branch of the optical as well as of any other trade or calling. It is hard to improve upon the old proverb, which, if I quote correctly, tells us that—

"One thing at a time
And that done well,
Is a very good maxim,
As many can tell."

In conclusion, I desire to suggest quite briefly that the optician who seeks distinction as such should take pains to provide that his place of business and everything pertaining to it is well appointed; that his display of articles is made in a manner suggestive of important work; and, above all things else (but this, of course, need not be said), that his own manner and treatment of those who consult him are at all times indicative of the polished mind of one whose calling should be classed as only next in importance to the learned professions. And, if I am permitted, I will offer the further suggestion that every optician should allow his equipment to include a library, as complete as possible, of works applicable to the branch of optics to which he particularly devotes his attention.

A well-appointed workshop also is what the proper optician can hardly dispense with. It need not be large, but it should be furnished with such tools and things as are necessary to perform an urgent piece of work.

Mr. President, someone has set me a task which I have endeavoured to perform as fully as time permits. I am conscious of my inability to present my subject in such a way as to provide enlightenment, but I realise and

appreciate the indulgence the meeting has given to me, and I hope that this gathering of opticians will, during what time is available, offer criticism of the remarks I have ventured to make. I also hope that what observations are made will establish some useful result in the minds of members of the Society and of those who are visitors, as well as perchance in the minds of others whose interests are, with the subject of my paper.
